

# Antiquity

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## Editorial Notes

THESE Notes are shorter than usual not from lack of copy but for the opposite reason—to allow more room for articles and reviews. We hope to return to the normal three pages in the next number. Making up each number of *ANTIQUITY* is always a problem of compression. We take this opportunity of apologizing to reviewers, authors and publishers for the delay in printing reviews; the only remedy is a larger circulation and the increase in size which that will make possible. A publicity campaign to effect this is now in progress, and we hope that any subscribers who may receive the leaflets will forgive us and remember that this is inevitable when one is distributing leaflets on a large scale to members of archaeological societies. We wish also to thank those societies who have helped by sending us lists of their members. It was upon such lists that *ANTIQUITY* was originally founded; the time has now come to bring *ANTIQUITY* to the notice of a new generation.

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The present number contains a new feature—‘ Important New Books ’. To borrow the words of a contemporary, the inclusion of a book in this list does not preclude its subsequent review. But we are sure that readers will like to know at once when such books are published. We shall include in this section articles that give authoritative accounts of important new discoveries and excavations. For the progress of knowledge is registered much more in the periodical journals and bulletins of learned societies than in books; and those who wish to keep abreast must consult them. The current numbers of such journals should be displayed on a table in the rooms of learned societies, but often they are not.

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This new feature was suggested by one of our most valued readers. We have often profited by such suggestions, and we invite our readers to write and tell us what they like, and also what they do not like, in *ANTIQUITY*. At the same time we are bound to say that we cannot always reply to such letters, for our correspondence is extremely heavy and we have no secretarial help. But we read all such letters and value them highly; they are a great help in every way. We hope that our readers have no grounds for complaint, either now or in the recent past; but if they have, we hope that they will also write and tell us, so that we may do our best to put matters right.

## 'Buried Landscapes' in Southern Italy

by JOHN BRADFORD

IT is widely known that war-time air photography has led to the discovery of many new archaeological sites of importance in Mediterranean lands. Many hundreds of tumuli have been added to the list, at such famous Etruscan cemeteries as Cerveteri and Tarquinia (1), and complete systems of Roman land-partition by Centuriation have been identified round the *coloniae* of Iader and Saloniae, on the shores of Dalmatia (2). But by far the most notable discoveries of all are those on the Foggia Plain, in the Province of Apulia, in Southeast Italy. Great numbers of Prehistoric, Roman, and Medieval sites are being identified, and some preliminary results have already been published in *ANTIQUITY* ('Siticulosa Apulia', December 1946). Select examples were exhibited at the Classical Conference at Oxford and at the British Association Meeting, in 1948, and again for several months this year, in the Ashmolean Museum. These were chosen from a number which it was fortunately possible to acquire for the University of Oxford, now housed at the Pitt Rivers Museum, where they are being studied in detail. This collection was based on vertical (3) photographs taken by the Royal Air Force, and oblique photographs taken by Major Williams-Hunt and myself (which were the first to reveal this dense concentration of sites, spread more thickly on the ground than almost anywhere else in Europe). This heavy concentration is of much more than local importance. During the last few years I have examined many thousands of air photographs of Southern and Central Europe taken at various seasons, in the course of my research. While these provide much interesting data and give us, as it were, an illustrated 'Domesday' survey of Europe in the middle of the 20th century (of capital value to Anthropology), in no other area has there as yet been anything approaching the quantity of crop-marks, grass-marks, soil-marks and earthworks which have come to light in Apulia. There are various reasons for this and a detailed account must await a later report. For our present purposes, it will be enough to single out one or two areas, for comparison. In the plain of Lombardy, the patchwork of little fields bearing a great mixture of crops has paradoxically made crop-marks very rare, and much the same is true of the valley of the Rhône and of large areas of Southern France. In Central Europe, the survival of a strip-field landscape has had the same effect, for the planted, ploughed, or fallow strips are so mixed that it is impossible to find a good-sized area with a single crop. In the Danube Valley, where one would hope for Neolithic sites complementary to those in Apulia, or traces of a Roman centuriated landscape, the chances have been minimized by the re-distribution of small-holdings to peasants (in the last century) in narrow strips aligned within a graticule of roads which is equivalent to modern centuriation, and irons out earlier traces. In other areas of potential importance, as for instance in the coastal belt behind Bari or Naples, almost continuous gardens of

<sup>1</sup> 'Etruria from the Air', *ANTIQUITY*, June 1947.

<sup>2</sup> 'A Technique for the study of Centuriation', *ANTIQUITY*, December 1947.

<sup>3</sup> The present illustrations are reproduced by kind permission of the Air Ministry, and are Crown Copyright. They were taken from heights between 10,000 and 25,000 feet, towards the end of May and early in June, and have been enlarged.



olives and vines severely restrict the possibilities of discovery from the air. However, even these are not entirely unrewarding.

But the reasons for the pre-eminent suitability of the rolling expanse of the Tavoliere di Puglia are quite straightforward and may well be met with again. They have been summarized in an earlier article, and it will suffice to recall the main factors. Saving its coastal marshes, this part of 'Siticulosa Apulia' has always retained the parched character it had of old. Throughout the Middle Ages, and indeed until comparatively recent years, very large areas were given over to pasture and untouched by cultivation, so that some big stretches of grassland have not been ploughed since the Roman period. The modern field-pattern that has grown up since the national state of the 19th century set about restoring agriculture to the region is made up of very large, open, arable fields. The conditions are ideal for crop- and vegetation-marks of all kinds, and earthworks have been allowed to fossilize in peace.

The topographical character of the region can be clearly seen from the scale-model, made and coloured by my wife and myself, shown on PLATE I (4). There is a natural unity about it, which favours our researches, although in Neolithic and Roman times it was economically linked with, and continued by, the coastal belt and inland uplands, to the south of it. The table-like plain of the Tavoliere is framed between the Apennines and the massive Gargano Promontory, and its natural boundaries to north and south are the Rivers Fortore and Ofanto, where the plain dwindles away. Let the reader imagine the setting: a buff and pale green landscape under a Mediterranean sky, shimmering in the intense clear sunlight, with belts of dark green round the huddled white towns; while the eye travels gratefully to the blue shady mountains which ring the plain. The sea is at hand and we look toward it, conscious that the opposite shores are near, and feel drawn in their direction rather than inland and up the Peninsula. Today, as in the past, this feeling reflects a real orientation in outlook and contact. The setting then is exhilarating and even harsh, not restful; for it is essential to capture the quality of the living landscape, before we consider those predecessors which are masked beneath it. The most important structural features, which have exerted a continual effect, stand out clearly in the model, which itself reproduces the nature of the terrain more effectively than does a map. The original is made in plaster of paris, working from a contoured and layered map, and has a horizontal scale of 1:250,000 (or 1 inch to 4 miles) and a vertical scale of 1 inch to c. 1700 feet, carefully determined. The whole covers a land and sea area of some 62 by 72 miles, and includes the Tremiti Islands, on which are Neolithic sites. From the point of view of Human Geography, the first low undulating slopes of the Apennines belong to the oval plain of the Tavoliere proper. Certain features have special significance, which have been brought out in the model, notably the 100 metre contour-line (an escarpment well defined on the ground) which often marks the limit of the Roman field-systems laid out by centuriation. Neolithic homestead and village sites, however, are found all over the plain and right down to the edge of the three lagoons, Lesina, Varano and Salso, with their marshes and dunes.

Many of the photographs show traces of successive periods, as a palimpsest more complex than even those to which the late Major Allen accustomed us in his discoveries in the Oxford region. When the Apulian photographs are seen, it soon becomes apparent that one cannot take an exclusive interest in the sites of any one period or kind. Such specialization would be out of place in the cavalcade of cultures thus disclosed. We are

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<sup>4</sup> We wish to thank Mr H. N. Newton and Miss O. Godwin, of the Ashmolean Museum, for their skill in photographing the model from different view-points.



watching, in effect, the stages in growth of a European peasantry, from its formation and over the last 4000 years. Anyone who knows Apulia well, or a similar peasantry, will also know how interbred are the present and the past, and that this continuity is not an abstraction. We need therefore an element of Anthropology in our Archaeology, here especially; and this means to say that our survey of Humanity from the air must also be Humanism from the air. The essential business of plotting the sites, measuring them, and comparing them must go forward, but we should always remember that these are the 'rough workings' as we move towards our human equations. To compare circles with circles, and squares with squares, is the elementary stage, though an indispensable foundation of fact. The human landscape is an organic thing continuously evolving through phases of growth and decay. In Apulia, the Neolithic and Roman pattern of settlement, so clearly revealed to us from the air, has vanished below the ground, but life and the same peasant stock continued. One could hardly wish for a better illustration of that idea of a sequence of 'Buried Landscapes', which Dr G. E. Daniel propounded in a recent broadcast.

It would not, in any case, be surprising that the largest area of plain in Italy south of the plain of Lombardy should be able to show many traces of settlement. Nevertheless there has often been a tendency to treat it as a remote backwater, an appendage to Taranto, or Naples and the Campanian plain. But the recent discoveries from the air strongly suggest that it deserves far more attention than it has yet received. Moreover these discoveries have a more than local application. The Prehistoric, Roman, and Medieval sites, present in abundance, have each in their several ways contributions of far wider significance to make to the archaeology and history of Europe as a whole, and these are dealt with below in turn. The necessity of following-up the photographic interpretation with field-work on the ground is well recognized, since the task of surveying and test-excavation is the necessary sequel, and complement, to all discoveries made by air-photography, and is accepted as axiomatic in this country. A failure to take this necessary and logical step would be a grave omission. Fortunately, however, an archaeological reconnaissance has already been approved in principle by the Italian authorities. Grants towards the cost have generously been voted by the Society of Antiquaries, the Prehistoric Society, the Craven Committee and other bodies, and, at the time of writing, I hope to begin a programme of field-work and test-excavation during the present summer under the auspices of the Apulia Committee set up through the Society of Antiquaries, and with the assistance of the British School at Rome.

#### NEOLITHIC COMMUNITIES

First we may summarize the conclusions set out in my earlier article. Over 200 (5) sites of 'village' and 'homestead' character have been found from crop-marks, some measuring up to 500 by 800 yards overall. This is one of the densest concentrations of prehistoric settlements yet known in Europe, lying as they do within an oval area of some 30 by 55 miles. These sites are all of cognate kind, and not to be confused

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<sup>5</sup> Probably this number would be doubled if further systematic air-photography was undertaken for short spells in early summer, over several years. We are reminded of the thick concentration of Neolithic settlement-mounds in Bulgaria, described by James H. Gaul in the *American School of Prehistoric Research Bulletin*, no. 16, 1948, p. 79. The 'vast' number of 220 have been recorded in Bulgaria, but this no longer seems so extraordinary in view of the quantity of sites on the Foggia plain. The great majority of those in Bulgaria are merely recorded, from ground observation, and thus there are very few plans. Gaul estimates a population of 5000-10,000, at any one time, in late Neolithic Bulgaria.



with any others seen in Apulia. They are all variations on a circular plan and are surrounded by broad ditches, from one to eight in number. Within them lie small ditched enclosures, of circular or penannular form, which are presumably 'compounds' containing dwellings, somewhat in the style of simple societies based on mixed farming found in parts of Africa today. The largest of these measures about 150 feet across, and the smallest about 40 feet. The mean overall diameter of the great majority is between 40 and 70 feet (including the ditch which is usually some 10 feet wide or more). In the 'homesteads' there may be no more than a handful of these, though in a large village 100 or even more can be identified. The diameter of the enclosed area is too large for the ditches to have played any part in the structure of dwellings within; and one may reasonably infer that they were intended to secure privacy, and perhaps also to keep out stray cattle or to act as windbreaks. Sometimes the circle is completed, in others it may have been rounded off with a portable barrier of hurdles or the like, which has left no trace on the photographs. It will be observed that some 'compounds' are much larger and more imposing than others. Whether this is a difference in status, in function, or merely in the number of persons living together, it is impossible to decide without the complete excavation of several of various sizes. But we ought eventually to be able to achieve some finality about the answer to this question. Within a given site, the opening appears almost always on the same side of the compounds. Again, at present it cannot be said whether this orientation is related to the position of the main entrance to the site, or if it was decided by custom, or even by the prevailing wind, or else lay conveniently in the direction of the principal pastures or tillage. Many other significant points can be collated directly from the photographs, but, to avoid repetition, it can be said that in the last two years further study has not made any change necessary in the details given in my earlier review, to which this forms a sequel. However there has been one important development which bears directly on our survey. This was the admirably clear and comprehensive classification of the pottery of Neolithic Apulia, by R. B. K. Stevenson (6), which goes a long way towards sorting out the successive cultural phases, and indeed, as far as is possible without further stratigraphical evidence. He suggests that ditch-enclosed settlements may make their first appearance alongside the fine wares arriving in his Phase 2 and developing in his Phase 3. General parallels for these wares are not lacking eastwards across the Adriatic, but it must be confessed that the exact area of origin proves somewhat elusive. His conclusions accord with the approximate date, rather before 2300 B.C., by which I suggested that the first of these ditched settlements had appeared on the Tavoliere. But if this is as yet vague, their end is still more so. Indeed, the degree to which we may think of them as continuing into the early stages of the Bronze Age is almost a matter of personal choice, and so it will remain until a little 'tell' like Coppa Nevigata, with its Neolithic, Bronze and Iron Age strata, is systematically sectioned. First, however, we need to know the span of life, and composition (with the total of storage pits, etc.) of a typical small Neolithic homestead. We now have the widest possible choice of site, and are forearmed with such ready-made site-plans as are seen on PLATE 2. In the test-excavation that is now so necessary, the surrounding ditches should be sectioned and related stratigraphically to the nearest clearly defined hut-enclosure, which ought to be stripped together with the ditch around it. This would provide material to define the dating of sites of this class, and also sample plans of huts and other structures which at present are quite unknown. This would be invaluable for the planning of future work, which logically should aim at stripping an entire

<sup>6</sup> In the *Proceedings of the Prehistoric Society*, 1947.



settlement in the manner of Köln-Lindenthal. The lessons to be drawn from the total stripping of the Iron Age farmstead at Little Woodbury, with its far-reaching repercussions on the social and economic history of its period and even on the demography of Roman Britain, are too recent to need further emphasis.

But there is much still to be learnt from the analysis and synthetic study of the corpus of settlements on the air photographs. Let us take for a moment, as laboratory specimens for examination, the representative sites shown on FIG. 1, and selected to show various recurring characteristics. They have been plotted from photographs of a scale of 1:15,000, and are here reproduced at a uniform scale for each. Plan A shows how close together they often occur, 500–800 yards apart, and how the superimposition of similar lay-outs indicates a long life and successive phases of re-construction. Plan B illustrates one of the most elaborate of the small sites which take advantage of the low escarpments round the plain. Portions of no less than 8 ditches, in two groups, are visible. There are numbers of such ‘cliff-castle’ types inland at the back of the plain. On Plan C is a typical village and a typical ‘homestead’ site. The concentric arrangement of the larger is only an emphasized form of the smaller. We do not yet know if there was a progressive growth in size, but I am inclined to think that both types were probably present from an early stage. The outermost enclosure of the smaller appears to have its counterpart in the similar feature (of uncertain extent) in the larger site. We do not know precisely how ‘mixed’ (7) was the ‘mixed farming’ of the Neolithic peasant in Apulia. This is one of the major problems to be solved by excavation. But, as already suggested, this outermost enclosure is probably a ‘home farm’, as it were, for pasturing herds at night or at particular seasons, or perhaps for fencing off cultivated garden land (no traces of internal field-boundary ditches have been seen). Plan D shows a settlement whose two inner ditch-enclosures closely resemble in size and plan those of the Neolithic village of Köln-Lindenthal. An escarpment site of the simplest sort is adjacent. In Plan E, which for its great area is extraordinarily complete, we have one of the finest examples of the village sites, which if it had possessed the powers of self-reproduction would verge on the proto-town. An oblique air photograph of this village taken by Williams-Hunt and myself, and a vertical for comparison, have already appeared (8). The settlement measures *c.* 500 by 800 yards overall, to which must be added another 700 yards for the outer ‘home-farm’ enclosure. Within are a hundred or more ‘compounds’ of varying size. It lies on level ground about 8 miles NE of Foggia, and, as with a number of sites, some little distance from a stream. It is common to find a small enclosure outside and adjacent to a large settlement, but since they are so thick on the ground this may have no significance. The ditches round the village are 15–20 feet across (measuring from the crop-marks which give a reasonably accurate idea, as I have tested by excavation). Their siting in pairs is a frequent feature, as will be seen on FIG. 2. On Plan F, we have a good-sized village following the contours of a low hill, but the ‘compounds’ inside are insufficiently clear to plot their positions, and their absence from some areas in the other sites is likewise due to crop-conditions. It would appear from Plan G, which resembles both Murgecchia and Murgia Timone, that we have

<sup>7</sup> Compare V. G. Childe, *Dawn of European Civilisation*, 1947 ed., 226.

<sup>8</sup> In ANTIQUITY, Dec. 1946, 196 seq., and Plates II and III. In 1945, I was able to have a trench cut through the ditch of a hut-compound inside a similar large sub-triangular village site. This produced, right down to the bottom of the square-cut ditch, a large quantity of the characteristic fine burnished wares and some painted pottery (decorated ‘*a fasce larghe*’), typical of the Apulian Neolithic, and both falling within Stevenson’s Phase II. The ditch measured 7 feet in width at the top, and was 5 feet deep.

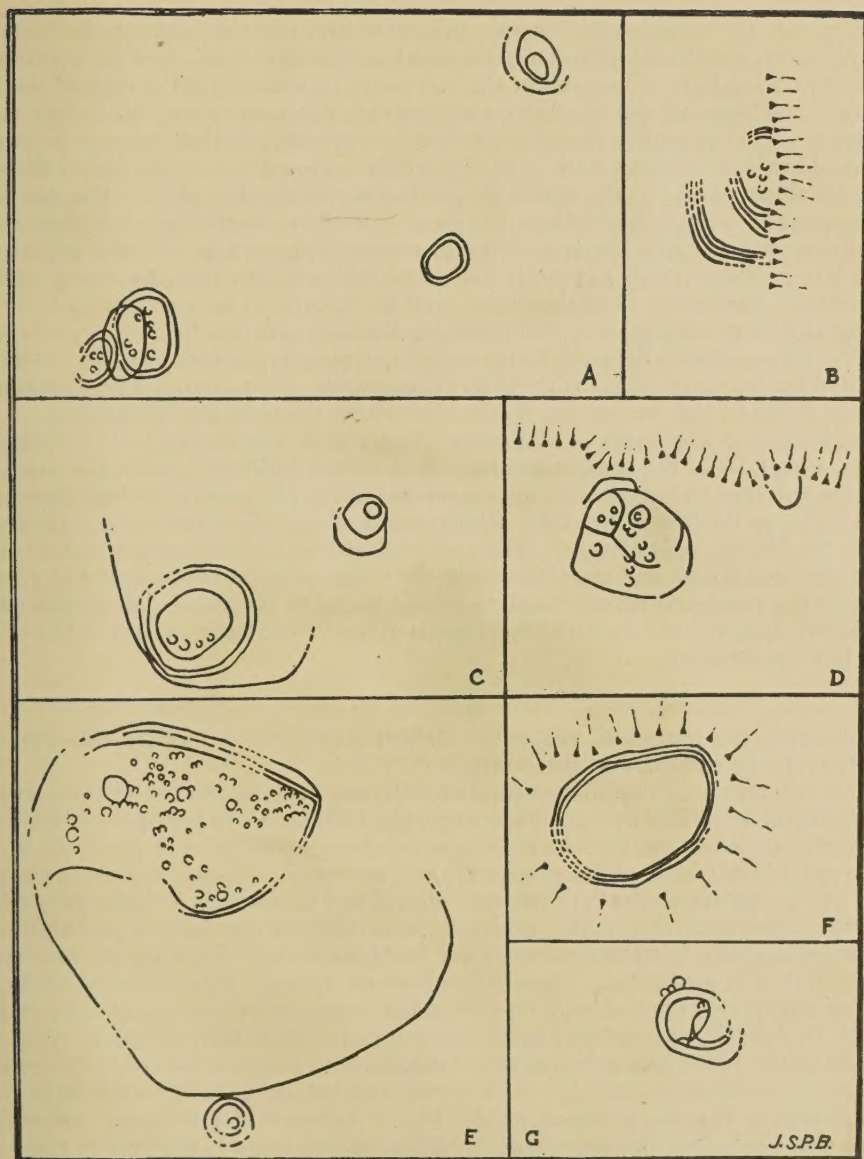


FIG. 1. COMPARATIVE PLANS OF DITCH-ENCLOSED HOMESTEADS OF NEOLITHIC TYPE AND OF VILLAGES WITH THEIR HOME-FARM ENCLOSURES; taken from air-photos, of the Foggia Plain and plotted on a uniform scale of about 1 : 20,000 to show the principal varieties of site



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a small group of 'compounds' outside the enclosure, but this seems to be very rare. Finally, on FIG. 2 we have a representative area from the prehistoric landscape, 5 miles NE of Lucera, on level ground on top of the 100 metre escarpment at the back of the plain. There are two large villages of similar plan and only 800 yards apart, which may well be successive. These measure some 800 yards across overall, and the 'compounds' seem to be confined within the inner pair of ditches which are some 15-20 feet wide, and for the most part about 50 feet apart, with 250-400 feet separating the pairs. The number of hut-compounds is much larger than it is possible to show clearly here. A 'homestead' type of site, with a typical example of the escarpment-edge siting, and the partial plans of two lesser villages (each 250 yards across overall), are also seen, besides a scatter of small circular enclosures (and there may well be more). The curving trackway, too, showing as a crop-mark (C-D-E), follows a pre-Roman route (see below).

The extraordinary degree of pre-excavation planning which can be obtained is illustrated by the small village with its hut-compounds, on PLATE II. These compounds are 14 in number, and we can see that we have here the total for the whole site. The area enclosed is c. 240 yards in diameter. The village ditches are 12 feet wide on an average, and those of the compounds from 8 to 20 feet (probably where re-cut). As is usual, the entrance to the village is not prominent in the plan, and is perhaps represented by the bulge in the line of the ditch (shown at A), if we follow the analogy of Ridola's sites on the Murge, near Matera. The area enclosed by the compounds varies between 40 and 100 feet across, and the area within the village ditches covers roughly  $9\frac{1}{2}$  acres. A smaller site nearby, is revealed mainly by soil-marks in vineyards. A general picture of scale in relation to the modern landscape is given by the large courtyard farm, in the upper left-hand corner.

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Following the procedure proper to analysis: to collect, compare and generalize, we now arrive at the third of these stages.

As we fly across the dry dusty plain, in its summer colours, and survey the beginnings of settled community-life over 4000 years ago, it is difficult not to feel a pang of admiration for these methodical, sturdy and ingenious peasants whose labours prepared the way for the evolution of cities and nations. They were orderly revolutionaries on the grand scale. A 'buried landscape' of primitive villages and homesteads is continually opening up to the view, and we examine this with something of the feelings and the precise familiarized eye of a District Commissioner in Africa today. Here are the first farmers in South Italy—more than 2000 years before Imperial Rome. By any reasonable analogy, it is unthinkable that the formation of the peasant character of this region was not deeply affected, far into later periods, by these six centuries or so of early farming. It is of the greatest interest to see how far these large villages could progress toward towns with very little, or no, metal or metallurgy; and as material for the sociology of prehistory they offer fascinating, if as yet untested, possibilities. As was explained in my earlier article, we may now infer a distribution of such Neolithic communities of fair density southwards over the Murge uplands, the Salentine peninsula (the 'heel' of Italy) and down to Taranto.

It will be seen that these discoveries stimulate as many questions as answers, and this is all to the good. One may speculate, for example, on the nature and function of the most distinctive feature of the group as a whole, namely their concentric ditches (for only a handful of *unenclosed* hut-compounds have been seen, and even these are in some doubt). They would have been appropriate to the new settlers, and needful



thereafter in a well-peopled but exacting region, in which competition for the better land must have been continuous. But the multiplication of the ditches (presumably palisaded) leads one to ask what considerations of tactics may have prompted it. Not many slingstones (9) have so far come from Neolithic sites in South Italy, but this is not conclusive for it required Professor R. E. M. Wheeler's systematic excavations to establish the relation of sling warfare and multiple ramparts in our own hill-forts. The 'homesteads', too, are a problem. Almost all show the combination of the 'domestic' and the 'farmyard' enclosures in their plan, concentrically or opening out of one another. Yet are a number of these temporary group-sites for seasonal occupation? It is impossible, as yet, to define the type socially or economically, but it must correspond to some particular form of human grouping (? the 'enlarged family'). They are freely spread all over the plain and not confined to a particular type of siting. The definition, by excavation, of one such site as an identifiable social group is an essential step in any future work, for generalizations on the social status of unexcavated sites are insidious and too easily accepted as facts.

But we may suggest that it was from this time, and in such a setting, that the familiar European combination of village and homestead originated, and the landscape begins to wear an aspect not unfamiliar. We must envisage it, too, against the background of the continuum of Neolithic peasants from Black Sea to North Sea. In spite of the regional variations which we can detect there is an over-riding and generic unity of culture transcending local differences. The study of these communities in South Italy is, therefore, bound to have a not inconsiderable effect on what we may term the 'international Neolithic'.

#### ROMAN FIELDS AND FARMS

The Roman landscape, which we are now able to reconstruct in outline, can be clearly distinguished on the photographs in its essentials. Since there is such a quantity of new material, we must be content here to dwell on certain of the more important features, and relegate much to a later and fuller treatment.

There are three topics of primary importance, which we shall deal with as follows:—

(1) Several elaborate systems of Roman land-partition by centuriation have been revealed, on the west and south sides of the Tavoliere, by the crop- and grass-marks of the boundary ditches. The *centuriae* (the square units of division) vary a little in size and orientation as between one system and another; but in all cases were demarcated by ditch-flanked roads. This chess-board landscape, which so vividly brings to mind a Tenniel drawing in the second chapter of '*Through the Looking Glass* . . .', is of the kind still well preserved in the existing field-pattern in parts of the North Italian plain. But *there* this incorporation in living field-systems has diminished its archaeological value. Continued use has preserved it but taken most of the life out of it, and only the bones are left. In Apulia, we are dealing with a Roman agricultural pattern which went out of use, and has been kept 'on ice' for us, as it were, below the ground. We cannot say, without excavation, exactly when these systems were laid down, nor in what manner and when they finally fell into disuse, vanishing from the surface. To piece together these systems from the traces shown on several thousand air-photographs requires both time and patience. It is apparent that separate and distinct areas of *agri centuriati* can be assigned to Roman cities round the plain.

(2) For the first time we are able to study the Roman farm-sites which lay at intervals along the roads, much as do the present State-planned farms in the same area.

<sup>9</sup> See *Bull. di paleontologia italiana*, XLVI, 152.



More than a dozen have been identified, and this number should soon be substantially increased.

(3) To the *centuriae*, which are the basic units of the land-division, we can now add the private field-boundaries within them, and even the pattern of the actual cultivation-trenches and pits, inside. The particular use of the latter can be deduced from size and arrangement, and by comparison of the lay-out with that recommended by such classical agronomists as Columella. A great acreage is covered by the patterns of these cultivation-trenches, and the problem of recording the detail is one new in archaeology.

We may conclude, therefore, by observing that never before have there been comparable opportunities for the study of Roman agriculture, *as actually practised*, or for seeing how far the agronomists' precepts were heeded, or for bringing a less rarified atmosphere, and some honest country mud, into the unresolved and abstract discussion of Roman professional terminology which has so often vexed the study of Centuriation. It has never before been possible to examine a Roman centuriated field-system 'three dimensionally' from surviving traces. Given this mass of new and detailed information a planned programme of excavation could be economically performed, with the assurance of important discoveries, and with an interest all the greater from being at the heart of the Roman World.

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We must now turn to illustrations of these fundamental elements of the Roman landscape in North Apulia.

On FIG. 2, an area 5 miles NE of Lucera (*Luceria*), will be seen the junction of parts of two systems of centuriation, differently orientated and with the parallel ditch-flanked roads differently spaced. That on the left belongs to the assigned *ager* round *Luceria*, which became a colony of Rome as early as 314 B.C., as a strategically-placed thorn in the flank of the Samnites. In Augustan times it ranked as a *colonia*. Only the East-West roads (the *decumani*) seem to have survived clearly. They have a constant width of c. 12 feet and ditches c. 5 feet wide on each side, and in the area round *Luceria* average between 550 and 600 yards apart (or approximately 15 *actus*). The curly road which forms the joint boundary (C-D-E) is far from what one associates with the rigid gridded landscape of which we normally think. It measures c. 40 feet wide (including the ditches, one of which has been re-cut); and is very possibly of pre-Roman origin, chosen as an existing feature when the boundaries of the *agri centuriati* were decided upon. Careful search established that, here, the centuriation was not continued north of, and below, the 100 metre escarpment.

At this point we must refer to what has been done to collate the observed systems of centuriated fields on the photographs with those which are recorded as actually having existed in those invaluable agrimensorial writings entitled *Liber Augusti Caesaris et Neronis* and *Balbus Mensor*, otherwise known as the *libri coloniarum* or *regionum*. Pais (10) has analyzed their entries in detail, and assessed their value much higher than did Mommsen but further consideration of the difficult problems of textual criticism must be omitted here. It is enough to say that the Roman sources give a number of details about the way in which land was divided round the towns in Apulia. In the preliminary interpretation of the air photographs, made some years ago and before consulting the

<sup>10</sup> *Storia della Colonizzazione di Roma Antica*, vol. 1, 1923, esp. p. 4 and p. 156. For a further discussion, see Rudi Thomsen, *The Italic Regions from Augustus to the Lombard invasion*, Copenhagen, 1947. His conclusions are of some value to our topographical researches, which we shall later collate with the written sources and inscriptions.



## ‘BURIED LANDSCAPES’ IN SOUTHERN ITALY

*libri coloniarum*, I noted centuriated field systems with significant differences in the orientation of their units in the neighbourhood of the following former Roman towns :—Troja (*Aecae*), Ortona (*Herdonia*), Ascoli Satriano (*Ausculum*), Canosa (*Canusium*) and Lucera (*Luceria*). The positions of these towns, on the first low escarpments at the back of the plain, will be seen on PLATE I A. It is of particular interest that these towns *are*, in fact, mentioned in the *libri coloniarum* as having their land regularly laid out, and

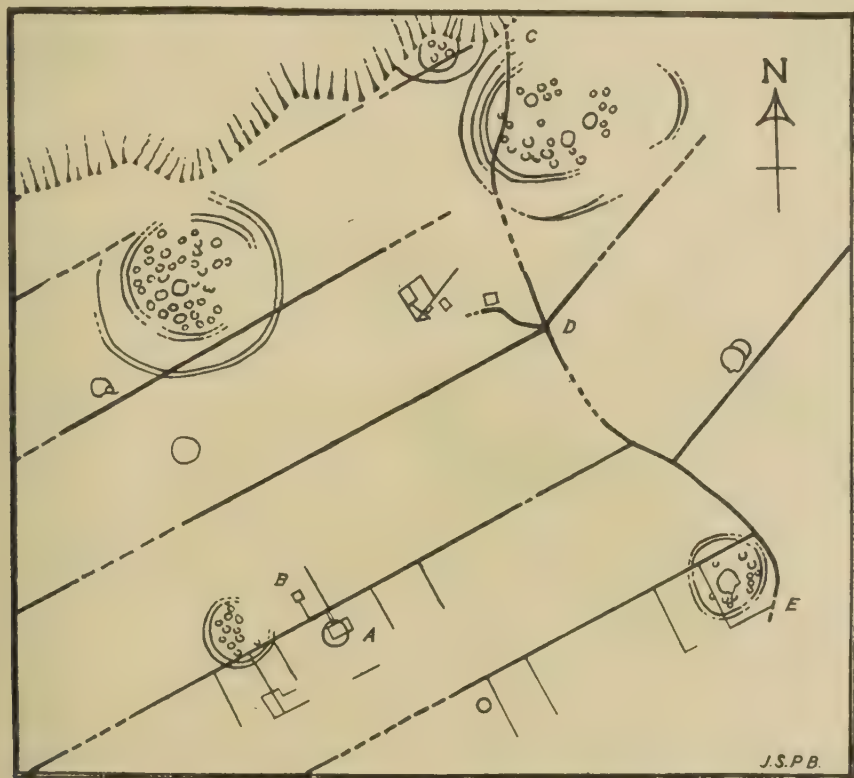


FIG. 2. AN AREA 5 MILES N.E. OF LUCERA, SHOWING A SPECIMEN AREA OF CROP-MARKS PLOTTED, WITH CIRCULAR NEOLITHIC VILLAGES UP TO 850 YARDS ACROSS AND THE BOUNDARY BETWEEN TWO ADJACENT SYSTEMS OF PARALLEL CENTURIATED ROMAN ROADS

Scale about 1 : 26,640 (1 inch = 740 yards)

details of their centuriation are given in several cases. But no trace of centuriation has yet been seen, on the photographs, between Foggia and the L. Salso (PLATE I A), and there is none round the site of *Arpi* (nr. Foggia) which in the 3rd century B.C. was one of the most important cities of Central Italy. Of the situation at *Sipontum* and *Salapia*, I cannot yet speak with certainty, but this preliminary statement on the collation and identifications already made may be of use. We now have in front of us the lengthy process of accurately positioning the constituent fragments of these centuriated field-systems, spread over many map-sheets of the 1 : 50,000 series.

Next in order we may illustrate the farms. Let us first consider the two on PLATE III A (for their location, near Lucera, see FIG. 2, at A and B). These are connected with the Roman centuriated road by ditch-flanked drives. The larger (A) has a subdivided rectangular enclosure (490 by 150 feet), with an irregular dark central patch which may represent the site of a building. It is nearly superimposed on a circular ditched enclosure of prehistoric character (not *all* of the simple circular enclosures are necessarily Neolithic). On the opposite side of the road, at B, is a much smaller Roman enclosure, about 60 by 60 feet within. Several such enclosures have been identified just off the roads and connected to them, and they may possibly be equivalent in function to those little isolated buildings which today are found in this area among the vineyards, and are lived in temporarily during the vintage or occupied by a watchman who tends the grapes. This suggestion finds support from the fact that immediately behind the enclosure (at D) are rows of parallel Roman cultivation trenches, set 10 to 12 feet apart from centre to centre, which we have good reason to believe were for vine cultivation, as we shall see. There is another distinctive group of these at C.

We may take our review of the farms a stage further with PLATE III B. Here, once again, we have a Roman enclosure (a parallelogram 108 feet square internally, with a ditch 6 feet wide) (11) aligned parallel to a ditch-flanked centuriated road. Entrance gaps are visible at A and B. And *again* a circular enclosure of pre-Roman character occupies the same site. It is hard to reject the idea that some kind of continuity is in evidence in such cases. As before, the ground is flat, and there seems no other particular reason why these sites should be superimposed. If this could be confirmed by excavation the implications for the history of this region in the early period of Rome's authority would be of considerable interest; moreover, in the preceding centuries, the human landscape outside (12) the townlets is still dark indeed. It should be noted that, close at hand, there is a large area of cultivation trenches (6 to 8 feet apart from centre to centre) probably a vineyard adjacent to the farm, and aligned on a ditch connected with it. The large dark patch over it is due to modern stubble-burning, and the tracks of motor vehicles can be seen on the right of the photograph. With practice one can eliminate such extraneous features from further consideration.

Dissecting our buried landscape still more minutely we come next to the cultivation trenches and pits. Here is one of the most interesting aspects of all, for we are dealing with the Archaeology of the commonplace, indispensable but elusive and very liable to go unrecorded. It is through its attention to such detail, and its ability to reveal it, that 'dirt archaeology' can help the writing of classical history to take the next big step forward. These features are the works of the innumerable peasantry and yeomanry on whom the staple cornfields, vineyards and olive groves depended. It is right to recall those words of Professor R. E. M. Wheeler: 'The detailed history of an African farm recovered by careful excavation would throw a flood of light upon the chequered economic life of this vital province'. No less, it must be emphasized, is this true of Italy itself.

An examination of PLATES IV and V A will reveal the kind of detail that is awaiting investigation. On PLATE IV is visible (at A) the intersection of two roads (*limites*) delimiting

<sup>11</sup> i.e. about 1 *actus* across. One is reminded of the small Roman farm sites, enclosed by rectangular ditches, found from the air in Oxfordshire by Major Allen (see *V.C.H.*, Oxon., 1, Plates XXI-XXIII).

<sup>12</sup> As T. J. Dunbabin has pointed out in *The Western Greeks* we know very little indeed about this aspect, and the social picture thus tends to be unbalanced.



the *centuriae* (13). These roads are *c.* 10 feet wide, with ditches *c.* 4 feet wide on either side. At B, exactly aligned along the road and unquestionably associated with it, is a large block of small regular crop-markings in parallel lines which represent elongated pits. About 36 rows can be seen with some 30 pits in each. These are each some 10 feet long, with an interval of *c.* 6 feet between those in each row, and a gap of *c.* 8 feet between the rows themselves. Another grouping of the same kind, across the road, in the same alignment and of greater length, can be identified on the original photographs but is obscured by modern agriculture. The spacing is rather too close (14) for olives, and this specialized pattern suggests a vineyard, orchard, or nursery plantation. We have never been really clear about the means of supplying the towns with their vegetables and fruits which played so important a part in provisioning the table. The holes had to be dug to take the tap-roots down to the subsoil, and perhaps fell to the lot of the *fossor*, the slave who specialized in digging by hand, not ploughing (15). Various crop-markings can also be seen at c and d.

PLATE V a displays another cultivation pattern, and one which covered large areas within our Roman centuriated field-systems. Again, it is fitted into the angle of intersection of two *limites* (at A). Here, the cultivation trenches are continuous lines, but stop just short of the ditch along the road, and a pathway (a feature advised by Columella) through them is clearly seen (at B). Areas of unsown land, and dark patches perhaps due to weeds, obscure the crop-markings somewhat, but enough is visible. The trenches are set *c.* 8 feet apart and measure *c.* 3 feet in width. The parallel with the modern vineyard, is very close; examples of the latter are indicated (intermixed with some olives) on PLATE II, at B. The opportunity of directly comparing the actual facts empirically by excavation with those principles of planting advised by Columella, and others, needs no stressing. In writing on the proper methods for planting vineyards, he recommended (Book IV) trenches  $2\frac{3}{4}$  feet deep. These would certainly provide us with crop-marks, today. He suggests various methods:—'Some set all their vines at 10 foot intervals in the form of a quincunx (16), so that the ground may be broken up by diagonal and cross ploughing in the manner of fallow land'. 'But those who dread the expense of trenching the ground, and yet wish to imitate that trenching in some measure, run straight furrows to a width of 6 feet leaving alternate strips of equal width; then they dig the furrows and deepen them to 3 feet, and place the vines or shoots along the sides. Some make a furrow  $2\frac{3}{4}$  feet deep and 5 feet wide; then leaving three times as much unbroken ground' they cut another (Book III, XIII).

In conclusion we must urge that the evidence from the air-photographs goes far to show that there has long existed some misconception about the nature of Roman economy in this region of Apulia. The Tavoliere has indeed become accepted as a classical instance of Roman land given over to pastoralism and seasonal transhumance of flocks between the plain and the mountains. This may well still be true of the area which as

<sup>13</sup> For their position on the map, see the vertical view of the scale model (Plate I, at I, near the foot of the Gargano). They would be part of the centuriated fields 'quae circa Montem Garganum sunt', mentioned in the *libri coloniarum*.

<sup>14</sup> A modern olive grove can be compared at E.

<sup>15</sup> For his activities, which included the deep furrows for drainage (*sulci*) mentioned in Justinian's *Digest*, see M. Maxey, *Occupations of the Lower Classes in Roman Society*, Chicago, 1938, 79–80.

<sup>16</sup> An arrangement in groups of five. Any five in the same position form a square with the fifth at the centre. Examples of this have been found on the Apulian air photographs.

yet shows no traces of centuriated cultivation, between Foggia and the Lago Salso, but the picture is now very different for other areas of the plain and for the gentler slopes behind it. Here we find centuriated fields and abundant areas of cultivation trenches. It is indeed a warning against building arguments upon scrappy written evidence and inference. Apulia had been labelled pastoral, and this idea has persisted through many authorities (17). In her *Geography of the Mediterranean Region*, E. C. Semple wrote that in Northern Apulia, between the Apennines and Monte Gargano, the subsoil was impenetrable to the roots of trees, and that though 'useless for orchard or vineyard, it served well for pasture land' (p. 326). We can now go far towards peopling this landscape, not in a sand-table world of theory, but in a setting of actual fields and farms which provide unrivalled opportunities for the direct study of Roman agriculture.

### THE MEDIEVAL LANDSCAPE

The important part played by Pastoralism in this region in the Middle Ages is well known. Archaeologically this can be studied not only in the *tratturi* which survive above ground (steadily being encroached upon by cultivation), but also in those already abandoned which show as crop-marks in the modern fields. These *tratturi* were broad tracks, State-owned, for the passage of immense flocks, and were demarcated on each side by a ditch. It is clear that the photographs can contribute much detail to this aspect of the archaeology of recent history.

However there are more important discoveries, for the medieval field monuments of this region are little known apart from those as well preserved as Emperor Frederick II's Castel del Monte. We have been able to identify 30 or more earthworks of medieval type, many of them quite large. Some are of motte and bailey construction. The history of the Normans and Hohenstaufen in this region is all too little known, and these earthworks should provide rich material. The need at the moment is for a thorough ground-survey to supplement the photographs, and for the documentation of the new sites from written record. In the latter, we have been most greatly helped by the authoritative knowledge of Miss E. M. Jamison. Since it is intended to devote special study to the medieval landscape of the Tavoliere, in a later article, it will be sufficient to illustrate the general nature of the new discoveries, as material for economic and social history. On PLATE V B is a site 13 miles NNW of Foggia marked on the map as Torrione del Casone, 'the large tower of the great house'. A large rectangular earthwork, with bank and ditch, is seen at A, measuring some 430 by 370 yards. Inside, besides a farm, can be seen buried traces of stone buildings, including the base of a tower. But the feature of special interest is the pattern of ditch-enclosed fields round about it. These are sub-rectangular in shape and vary from 50 to 450 feet across. They do not show traces of vineyard-trenching and so may have been intended for cereals. They are clearly associated with the radiating ditch-flanked trackways (C, D, E, F, G), which are also shown by crop-marks, but the large modern fields take an entirely different course.

Thus, for the first time we have the detailed plan of a medieval field-system, which at some later period went out of use and has thus been preserved in skeleton form, in the ground. With care and good fortune it may be possible to reconstruct the whole extent of the area round the earthwork that was thus intensively cultivated. The traces of these

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<sup>17</sup> It is accepted by E. H. Carrier in his *Water and Grass, a study in the Pastoral Economy of Southern Europe*, 1932, and has found its place also in M. Cary's recent book, *The Geographic Background of the Greek and Roman World*, 1949, pp. 140-1.



fields continue for three-quarters of a mile in several directions, and the cultivated area was thus quite considerable. This site is matched by others discovered on the Tavoliere. It lies only a mile or two to the south of the area of Roman centuriated fields at Casa Scoppa, shown on PLATE IV (the position is indicated on PLATE I A, at 1) on the level plain with the Gargano rising suddenly above it, magnificent in mountain wildness. Protected from the north and east winds, and well-watered comparatively, this corner of the plain gives a feeling of security, fertility and natural beauty, to which no early farmer could be insensitive. But this, of course, is also the very part of the plain which has always been thought of in terms of medieval pastoralism and almost its special preserve; although we knew from the documents that some villages did exist and that there were constant litigations as the peasant cultivators fought a losing battle against the encroachments of a State-sponsored pastoralism. Now, however, a re-interpretation of the economy and society of the medieval Apulian landscape will have to be undertaken, in the light of the new facts, and with much greater emphasis on its cultivation. We are in fact faced with an entirely new situation. As will be seen on PLATES V B and VI, this evidence can only be brought to light, in the first instance, from the air. Cut the crop, and you remove it until the next spring season, like a well-cleaned slate. But once recorded on the photographs, the course of the ditches and other features can be followed up on the ground, and excavated for dating material in the usual way. The study of the medieval pottery of this region is in itself a new subject, and trial-excavations at a site of the kind illustrated should provide us with an invaluable stratified type-series. At first it might be difficult to date, but everything has to have its beginnings if we really want new knowledge, and the situation should encourage rather than deter.

Now, let us turn to consider PLATE VI. This site is  $3\frac{1}{2}$  miles south of Foggia, and its position is shown on the vertical scale-model (PLATE I A, at 2). The farm within it has perpetuated the name of San Lorenzo. As will be seen, it is a large earthwork with bank and ditch (still wet in places, to judge from the dark tone of the vegetation that marks its course). There are three clearly defined enclosures, A, B and C, which appear to have well resisted agricultural levelling. The innermost enclosure (A) measures about 340 by 230 yards, and the whole earthwork is 800 yards long overall. Though so large, very little is known about its history, but Miss E. M. Jamison has most kindly conducted preliminary investigations in the documents to remedy this, and informs me that it is recorded that, in 1237, the Emperor Frederick II built a *palacium* or *domus* at San Lorenzo near Foggia, causing a village of the Bishop of Troja's to be removed from that spot. It would certainly appear very probable indeed that this, and the truly formidable earthwork whose plan has been recovered at the farm called San Lorenzo, just outside Foggia, are really one and the same. Any addition to our knowledge of the magnificent but enigmatic Emperor, that ‘Stupor Mundi’, would be welcome, and the life and character of his court and followers are still imperfectly known, although among the most interesting in the whole Middle Ages. Archaeological investigations here, especially of the buildings within, might yield information of great value (18). Around the earthwork is a network of roads (D, E, F, G) and ditch-enclosed fields revealed by crop-marks and soil-marks, all radiating from it. These can be traced for three-quarters of a mile in several directions, though some may have belonged to the earlier village. Modern boundaries take quite a different course (for example, the estate-boundary at x-y), and patterns made by harvesting, etc., have to be eliminated (for instance the field being cut, at z), but the small,

<sup>18</sup> Compare Professor Vogt's excavation of the Dark Age stronghold and Ottonian palace of the early Middle Ages at Zurich; see *Der Lindenhof in Zürich* (Füssli, Zürich, 1948: 30 Swiss francs).

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sub-rectangular, earlier fields are distinctive (19), and there is no danger of confusing them with others (e.g. the centuriated Roman landscape). It is of particular interest that certain recognizable features re-occur in the plans of several of the large earthworks now made known. One of these is the protective hornwork, U-shaped, which is seen on PLATE VI, at C, as an earthwork, and on PLATE V B, at B, as a crop-mark, and is met with in other sites of the same kind. It is possible that we may have here an identifiable class of site, perhaps of 13th-14th century date. There are others of a smaller, motte and bailey, type which may fall within the period of Norman rule, but these must await the fuller account that is being prepared.

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<sup>19</sup> Parallels given by Medieval illustration are suggestive, and my wife, who is investigating this, has drawn my attention to the general similarities with the field-patterns in the landscape backgrounds in paintings and manuscripts of the period. See, for example, Pope-Hennessy, *Sienese Quattrocentro Painting*, plates 19, 22, 28 and 29, and particularly the calendar illustrations to the *Très Riches Heures* (1904 ed.), prepared for the Duc de Berry, including plates III, VII and XXXV.



# Problems and Policies

by F. T. WAINWRIGHT

PERHAPS at no time more than the present have British archaeological studies had such need of a stocktaking' (1). An occasional stocktaking of knowledge and techniques is essential to the successful advancement of any study, and the Council for British Archaeology has undoubtedly stimulated archaeological studies by the publication of the first part of the *Survey and Policy of Field Research*. Under the joint-editorship of Professor Hawkes and Professor Piggott many of Britain's archaeologists have co-operated to review the present position and the future development of British archaeological studies. The volume is divided into two chapters; the first surveys our archaeological knowledge, and the second indicates how the major problems may best be tackled. Its span in time appals the mere historian. It ranges from the Palaeolithic Age to the 7th century of the Christian Era, from the so-called 'eoliths' to the so-called 'Kentish jewelry'. No single scholar would have been competent to discuss all the problems raised, and no single scholar is competent to criticize the work of this team of specialists.

The *Survey and Policy* is, by definition, archaeological. It is not the function of its authors to deal with kindred studies such as geology, dendrochronology, pollen-analysis and, later in the time-scale, place-names and the literary sources. Yet throughout the work one is conscious of the interdependence of these studies. The emphasis may shift from age to age, but the full picture of the past will always elude the single-minded approach of the single-minded specialist. This is extremely well illustrated in the earlier sections where the problems are as much geological as archaeological. It is equally well illustrated in the section devoted to 'Anglo-Saxon England' where the inevitable exclusion of literary and linguistic evidence leaves us with an archaeological residue that is 'surprisingly slight'. Though bound to limit their attention as far as possible to archaeological knowledge and to archaeological problems, these specialists lose no chance of driving home the vital point that pictures of the past are many-sided and that different techniques are required to reveal their different facets.

The ignorant layman and the semi-ignorant spectator cannot fail to be impressed by the masterly summaries of knowledge which are laid before them. Some summaries are more attractive and more convincing than others. Perhaps the most useful are those that list most possibilities and fewest facts, for at the present stage it is not wise to close too many doors even though the layman may cry for certainties. Masterly, too, is the discussion of the chief problems in British archaeology; concrete proposals are made for their solution and, occasionally, practical recommendations on method are added. Enough is said to sketch the lines along which the problems may be attacked, but not enough to encourage the untrained amateur, if he needs any encouragement, to indulge in an orgy of destructive digging.

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<sup>1</sup> *Survey and Policy of Field Research in the Archaeology of Great Britain*, Part 1, p. 9 (published by the Council for British Archaeology, London, 1948).

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Throughout this volume excavation-policy is discussed with a caution that is wholly admirable. It is agreed that certain sites should be preserved intact and unexcavated, for it is recognized that a programme of excavation should take into account the limitations of present knowledge and techniques as well as the relative importance of the problems themselves and the menace of destructive forces in an industrialized community. And the untrained amateur is frequently reminded, directly and indirectly, that unskilled excavation, however enthusiastic, is a dangerous and a damnable thing. The need for trained and experienced supervision of excavation is stressed, and the urgency of this need deserves special consideration at a time when the professional-versus-amateur controversy, always smouldering, has recently burst into flame.

Apart from scattered references to the menace of the untrained, there is a careful introductory survey which deals with modern tendencies, especially the increasing complexity of these studies, as they affect the amateur. And one of the joint-editors has elsewhere delivered a statement on the functions of the amateur (2) which has evoked some bitter retorts (3). A certain amount of heat, not all reflected, has also been generated in local societies where the amateur element ranges from high authority to stupid ignorance. Every trained scholar, archaeologist and non-archaeologist, must surely support the demand that valuable and irreplaceable material be saved from destruction by the incompetent and the irresponsible. Archaeologists, historians and place-name scholars are all plagued by amateur frivolities until they wish they were nuclear physicists, a class honoured by the State, favoured by the Exchequer and apparently untormented by the cranks. But whereas the historians lose only their tempers (privately, one hopes) the archaeologists lose also their material. And that is serious.

It would seem, therefore, that the amateur and his place should be listed separately as a problem, but a proper definition of the term 'amateur' would remove much heat from the controversy and would leave behind only a harmless and amusing warmth. What do we mean, in this connexion, by 'amateur'? The distinction based upon the assumption that a 'professional' pursues as a livelihood what the 'amateur' pursues as an unremunerative hobby suffers from a double weakness. It was always a poor distinction, and it has become ridiculous with the introduction of a week of five short days for most 'amateurs' and with the rapid expansion of teaching and administrative duties for most 'professionals'. Very few scholars earn their living by scholarship alone, and very few become scholars merely in order to earn a living. The old livelihood test must go, and with it must go (in this connexion) the terms 'amateur' and 'professional'. We are not concerned with the 'amateur' and the 'professional'. We are concerned on the one hand with the trained, the skilled and the experienced and, on the other hand, with the untrained, the unskilled and the ignorant. We should not say 'amateur' when we mean incompetent, and we should not say 'professional' when we mean scholarly.

Archaeological studies are to-day so complex that no man—not even he who might devote twenty-four hours a day to it—can range authoritatively over the whole field. Specialization has come to stay, and that is both beneficial and inevitable. Even to-day, however, the 'amateur' can hold his own if he limits his field, if he acquires the skill, the training and the experience, and if he has the time, the patience and the ability to achieve a mastery of his subject and of its background. These are the criteria—not

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<sup>2</sup> Stuart Piggott, 'Archaeology and the Amateur' (*The Archaeological Newsletter*, No. 1, April 1948).

<sup>3</sup> See subsequent issues of the *Archaeological Newsletter*.



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whether or not one is constrained to contribute to the Exchequer for the doubtful privilege of calling oneself a 'professional'. The dividing line is not between the amateur and the professional; it is between the trained and the untrained, the skilled and the unskilled, the competent and the incompetent.

The best amateur and the best professional will be found together on the same side of that dividing line. It is true that incompetent professionals are comparatively rarer than incompetent amateurs, but that is no cause for either pride or shame. It has a simple and obvious explanation: the professional's career depends (to some extent) upon his competence as a scholar, whereas the banks and the breweries are unperturbed if their servants choose to be bad archaeologists instead of bad golfers. The controversy has generated a quite unnecessary heat. Let not the cries of the incompetent and the ignorant divide the ranks of the competent or destroy the recognized standards which have always bound together the good professional and the good amateur. If an attack be worth the effort, let it be directed by both against the ignorant and the incompetent in our local societies, who, not content to watch and to subscribe, try to impress their honest brethren with their own empty claims to scholarship.

It is often argued that the amateur, freed from the dread of having to pay for his mistakes, introduces a stimulating unorthodoxy into archaeological opinion. The amateur's claim to a monopoly of unorthodoxy might be disputed, but no one will deny the importance of reviewing frequently one's evidence, opinions and methods. Unorthodox approaches, when coupled with a mastery of technique and an awareness of its limitations, are both welcome and stimulating. But without that mastery and that awareness unorthodoxy is apt to be ridiculous. Any fool can be unorthodox, even original, but unorthodoxy and originality in themselves have only an amusement value. On this point, as on all others, there seems to be no quarrel between the professional and the amateur as such. If there is a quarrel it is against the blunderingly incompetent and the presumptuously ignorant. Not enough hard words have been directed against these classes which have too long confused themselves with those distinguished amateurs who hold secure places in the highest ranks of scholarship.

The amateur as such is no problem. The incompetent amateur is a problem when he destroys important sites or when he gains control of local societies which for the most part are composed of sane and cultured men and women who are sincerely interested in scholarship and the promotion of scholarship. Incompetent and irresponsible individuals can do much harm by wasting funds on trifling publications, by undermining standards and by generally misleading the public (which is usually too busy to distinguish between the genuine and the fake). Such individuals should be imprisoned or, preferably, buried. But they should not be elevated to the rank of major problems.

There are many greater problems, and some of these, already apparent in the first part of *Survey and Policy*, will surely dominate the second part. *Survey and Policy* is concerned with archaeological problems and with archaeological policy; non-archaeological studies, already important for prehistory proper, are vital to an understanding of the Dark Ages. This is clearly demonstrated by the later sections of Part I. A survey of the archaeological problems of the Dark Ages is urgently required, but its value will be limited. Surveys of non-archaeological problems are also required and, finally, a survey which co-ordinates and integrates the various problems and the various lines of approach. This is no criticism of the principles underlying *Survey and Policy*—a wider approach would be outside the scope of the Council for British Archaeology. On the contrary, the example set by the Council might with advantage be followed by other groups of scholars. It would be extremely useful to have the various sets of problems

surveyed and to have even tentative policies available for consideration and discussion (4). Unfortunately at present there is no organization to stand aloof from sectional rivalries and prejudices and to carry out a co-ordinating survey. A Council for or an Institute of Dark Age Studies is required. An Institute or a Council or an Academy of 'Dark Age Studies' (forbidding title), 'Anglo-Saxon Studies', 'Anglo-Scandinavian Studies', 'Anglo-Celtic Studies', or simply 'Pre-Conquest Studies'—the name is not very important. It would be wise to include Celtic studies, and it would be wise to avoid in its name words like archaeology, history and philology. But the chief thing is that it should exist to encourage and co-ordinate all lines of inquiry likely to lead to the elucidation of the problems of Dark Age Britain.

Dark Age problems may be divided roughly into three broad classes. There are the gaps in our evidence; there are the problems of technique, and there are the wider problems of interpretation which arise from the need to translate our different kinds of evidence into history. The second part of *Survey and Policy* (which, incidentally, will cover the Medieval and post-Medieval periods as well as the pre-Conquest period) will no doubt sketch the archaeological gaps in our knowledge with the same mastery of touch as that which is displayed in Part I. We shall probably be told, for example, how we may best obtain archaeological evidence of the expansion of the English settlements and of racial boundaries in Scotland, how we may find or recognize at least a few heathen Scandinavian burials, how we may add to our knowledge of weapons and agricultural implements, village sites, ecclesiastical and domestic architecture, industrial developments and artistic achievements. We may hope for new lines of approach to the study of fortifications and battle-sites. But many gaps will remain that no archaeologist will be able to close. There are political, social, economic, artistic, literary and linguistic problems which archaeology alone can never solve. Nevertheless, we should like to see these non-archaeological problems clearly indicated, and we should like to see laid down the broad lines of policies aimed at their solution. We should also like to see all these problems, archaeological and non-archaeological, brought together in a single discussion; but until some co-ordinating agency is developed the interested spectator must remain in his fog.

The second set of problems, those of technique or method, are too complex for detailed examination in a general discussion. Excavation embraces the primary, though not the only, techniques of the archaeologist. It is only after he has recognized his problem and selected his site that the skill and technique of an excavator are brought into play. Excavation techniques are undoubtedly capable of great improvement, but their development in recent years has been spectacular, and to-day they present perhaps the most encouraging aspect of archaeological studies. It is as inspiring to watch a master of excavation technique as it is distressing to watch a bungler. We cannot appreciate a master's skill by reading his reports; we must see him at work in the field and we must experience his difficulties. Without some experience of excavation one cannot hope to assist in the gradual perfecting of excavation technique; a historian may guide an archaeologist to a site and he may perhaps greatly assist in the defining of problems against their background, but the physical attack upon the site must be left to the trained excavator. Those who have taken the trouble to examine different methods employed

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<sup>4</sup> Something of this nature was attempted for place-name studies about twenty-five years ago, i.e. *Introduction to the Survey of English Place-names* (English Place-name Society Publications, Vol. I, Part I). In this series of chapters, which cover the various aspects of place-name study, the authors set out 'to state the present state of our knowledge and indicate the lines along which the possibilities of future progress lie'.



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and to watch sympathetically different excavators at work are perhaps best qualified to comment on technique. Such observers believe that, though there are many obstacles still to be overcome, the development of the technique of excavation is in safe hands.

Non-archaeological studies have their own techniques. The historian may be engaged, for example, in stripping off the layers that have gone to make up the Anglo-Saxon Chronicle. Or perhaps he is engaged in testing the authenticity of charter material. The aim is fundamentally the same, but the spade is no use. Different techniques are required, and they often demand a skill and a knowledge outside the normal equipment of the historian (5). The student of place-names, too, is engaged in stripping off layers of phonological development and in searching for underlying and earlier structures. These and other studies have made considerable technical advances in recent years; some day even the Anglo-Saxon Chronicle will be sufficiently stripped or dissected to enable us to see the relationship to each other of its various sections and its various manuscripts. Unfortunately the spectator sees hardly any of this game; full realization of technical difficulties comes only from active participation. All these techniques are concerned primarily not with the interpretation of evidence but with the accumulating, the checking and the defining of evidence. It is not easy to see how they can be practised effectively except by trained specialists.

The third and most formidable group of problems arises from the fact that it is ultimately necessary to integrate these various accumulations of evidence into a single historical synthesis. It is extraordinarily difficult to co-ordinate the work of specialists. As a rule the historian, the archaeologist and the philologist distrust each other's methods. The philologist will often denounce the historian as 'inaccurate' or 'unscientific', and what he has to say of the archaeologist is often as vivid as it is unprintable. The archaeologist, in his turn, may declare that the historian is too cautious to reach useful conclusions and that the philologist is too rigid to reach any conclusions at all. Sometimes the historian appears to stand between the two: in matters of general interpretation he may consider the philologist too cautious and the archaeologist too careless. One will appeal to his 'academic standards' and the other will appeal to his 'common sense', but the object of this pleasant wooing frequently finds himself the victim of a disconcerting cross fire of abuse. We have all heard in private, and occasionally in public, mutual complaints and denunciations uttered in bitter rage, academic indignation or sly geniality. That specialists distrust the methods of other specialists is a fact that must be faced.

This distrust is not universal, of course. There are historians not frowned upon by the archaeologists and there are historians who have secured a cessation of abuse, if not positive praise, from the philologists. There are philologists who willingly bring their special techniques to the elucidation of historical problems and who do not openly condemn archaeology as an unacademic pursuit. And there are archaeologists who are not unimpressed by the arguments of historians and philologists. There are even a few scholars who move, unmolested and even respected, in two of the three camps. Nevertheless the barriers exist, and there are similar if less obvious barriers separating the several narrower paths that lie within these three broad avenues of approach.

How has this situation arisen? It has arisen chiefly because each scholar is dealing with a different kind of data, and because each different kind of data requires for its interpretation different methods of inference, different rules and techniques. An instance

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<sup>5</sup> See, for example, Miss F. E. Harmer, 'Anglo-Saxon Charters and the Historian' (Bulletin of the John Rylands Library, Vol. 22, No. 2, October 1938).

of assimilation revealed by a place-name form, for example, is not the same as a piece of pottery, and neither is the same as an entry in a chronicle. In none of these cases is the full historical significance of the evidence either obvious or near the surface. The most one can do is to draw from each a tentative historical conclusion, and disputes often arise because each scholar brings his own methods to the problem and tends to criticize the more, or less, definite conclusions obtained by other methods. The gradual development of the techniques of attack (i.e. against a site or a manuscript or a collection of place-name forms) has not been accompanied by a comparable development in the techniques of interpretation. Indeed the specialization which has so greatly improved one set of techniques has thrown new difficulties in the way of the development of the other. The time has passed when a single scholar could range with equal authority over a vast period. He now knows he cannot speak with equal claim to attention on every aspect of even a short period. Specialization, however, has come to stay. It has brought great advances, and it will bring more in the future. But it has emphasized and increased the difficulty of integrating the different bodies of evidence.

Dark Age studies receive the full impact of these differences in evidence and technique. In later ages historical information is usually too precise to tempt the archaeologist to ignore or reject it. In the prehistoric ages he has a clear field because there is no historical evidence to be considered. Many non-archaeologists may feel that the archaeological material of Neolithic Britain, for example, is insufficient to bear the superstructure of interpretation often raised upon it, but they refrain from criticism because a protest would look like interference. In the Dark Ages it is different. Here there is historical information and, however lacking in quantity and in precision, it must be taken into consideration. One of the greatest problems in the study of Dark Age Britain is how to reconcile current interpretations of archaeological material with current interpretations of literary and linguistic material.

It is not wise to write of battles and conquests when one's only evidence consists of changing designs on pottery or changing customs in the disposal of the dead, and it is not wise to see political convulsions in vowel sounds. Political events frequently produce artistic, social and linguistic changes, but to infer the former from the latter is like shooting in the dark. For the Dark Ages at least it is also unwise to build theories even on the apparently explicit statements of chroniclers until these statements and their sources have been carefully examined. An entry in a chronicle is not the same as what we may loosely call a historical fact. It can be demonstrated as a fact, for example, that in a certain Cambridge college there is a certain ancient manuscript which contains an entry which records that in A.D. 571 a certain Cuthwulf defeated the Britons and captured four named towns. We are not thereby automatically entitled to assume that in 571 the West Saxons advanced for the first time into territory hitherto held by the Britons. Without an exhaustive study of the source we are not entitled to assume that Cuthwulf won a battle against the Britons in 571. We are not even entitled to assume that there was a battle or that Cuthwulf existed except in legend. An exhaustive study of the source is itself only the beginning of a process which, if ultimately successful, will permit a historical conclusion—never, it should be noted, anything so precise as a mathematical proof. One must be cautious. Some archaeologists (and some historians) too readily accept such entries without adequate examination. Others too readily reject them without adequate examination. The examination may require the application of specialized techniques or the application of techniques not yet developed, but no such difficulties can justify a premature assumption that a question is settled. A manuscript may be material, but the statement that it contains is not like a piece of pot; it cannot be picked



up, measured, washed, treated with chemicals and labelled. Apart from such rare oddities as explosive cigars and oranges of soap, the archaeologist's material is direct evidence not of a state of mind but of a 'culture' and of a person's practical skill and (perhaps) aesthetic interests. A statement in a manuscript, unlike a piece of pot, may be true or false: it may represent an accurate summary by a person who knew the truth or a guess by one who did not; it may even represent a deliberate attempt to deceive. It is direct evidence only of a person's state of mind, and (if we ignore the possibility of an attempt to deceive) a belief that a certain proposition is true is only indirect and often very weak evidence for its actual truth. Not all archaeologists have yet grasped these annoying but essential characteristics of historical evidence. The application of the wrong technique is as useless as the application of no technique.

It is unwise to propose a new series of political events from an insufficient quantity of the wrong kind of evidence merely because the right kind of evidence happens to be lacking. It is even worse to do this when it requires the rejection of the right kind of evidence merely because the latter is imperfectly understood or apparently irreconcilable. But disputes can also arise from attempts to bring two kinds of evidence into close association. It is obviously important, for example, that archaeological evidence and historical evidence should be brought together, but when the evidence itself is meagre and intractable it is easy to make hasty and unconvincing associations. The simplest forms of such attempts at association are attempts to identify known archaeological evidence of battles with known historical conflicts, known burials with known historical figures, and known archaeological material with known historical events. Both archaeologists and historians spend much time working out and writing up such identifications. When they are successful their importance is great, but unfortunately the evidence for an identification of this kind is not always convincing to all parties. What to one is a reasonable probability may be a wild and unsupported guess to another. The meanings of 'possibility', 'probability' and 'proof' seem to have become blurred in the minds of some writers. When an archaeologist has shown that a certain burial belongs to a certain age and to a certain type he has opened the door to a number of possibilities. It is possible that he may have found the grave of a known historical figure who died in that area at that time. But it is also possible that he has found the grave of a person whose name has escaped preservation in the meagre historical sources. There were rather a lot of people in the Dark Ages whose names are not recorded in charters and chronicles. A possibility is no more than a possibility until further evidence turns it into a probability. We can all think of cases in which archaeologists and historians have leaped across the chasms that separate possibility from probability and probability from proof. It does no harm to throw out a possibility, of course, provided that it is properly labelled, but any theory built upon it cannot be stronger and will probably be weaker than its dubious foundation.

The techniques of interpretation are indeed in their infancy. It is easy for the historian to condemn the archaeologist's conclusions, and it is easy for the philologist to condemn the unscientific slovenliness of both historians and archaeologists. But all have to adopt some kind of historical method in so far as in the Dark Ages there is historical evidence to be considered and in so far as all seek a historical sequence of cause and effect in human activity. When the archaeologist thus turns historian he should remember the complexity of historical evidence. The philologist, on the other hand, might be more patient if he remembered that historical and archaeological evidence defies a strictly scientific handling. The philologist possesses a body of rules to assist him in tracing the origin and development of his vowel sound, but each site and each annal present to the archaeologist and to the historian new problems which cannot be

approached through rules based upon other sites and other annals. Each is a law to itself, and results are correspondingly more tentative. Different techniques of attack, devised for different kinds of evidence, have made it more difficult for their exponents to overcome the problems of co-ordination.

In the problems of interpretation and co-ordination a specialist can learn from other specialists. Philologists have developed a technique and they practise it. Historians will profit from a study of philological methods, but they cannot hope to apply philological precision to historical evidence which, incidentally, includes historical deductions from linguistic and archaeological material. Therefore historians have developed their own techniques—it's a pity they don't use them more often. The archaeologist can learn much from history and philology, but in the last resort his technique must be his own because, even though he seeks a historical synthesis, his material is archaeological, not literary, linguistic or historical. It is clear that the nature of the material influences both techniques used in examination and techniques used in interpretation.

The archaeologist needs help not abuse. His chief problem at the moment is how to develop for the interpretation of archaeological evidence a technique which recognizes the distinctive characteristics of archaeological evidence and at the same time permits the incorporation of his conclusions in a convincing historical synthesis.

It is important that specialists should understand each other's techniques and each other's difficulties. In the Dark Ages each is brought into close contact with the other's material, and blunders often result from attempts to handle unfamiliar kinds of evidence. The historian and the archaeologist may go astray if they attempt to use place-name evidence without first considering its limitations, and the philologist may misunderstand archaeological evidence unless he understands its nature. One will never be able to dictate to the specialist in his own field, of course, but one should try to understand his problems before trying to use his material. It might be argued that the specialists should be left to pursue their own lines in their own ways and that co-ordination should be limited to exchanges of questions and answers. Little would come of that. We all know how unintelligible and how irritating both questions and answers can be. An archaeologist who seeks help from a philologist without understanding his techniques and difficulties will infuriate him with questions that are either ridiculous or unanswerable. And the philologist, however anxious to help, will produce answers that are useless unless he understands the archaeologist's problems. We have all suffered from each other. A sympathetic understanding of the other man's technique is essential before one can ask him intelligible questions or give him intelligible answers. This seems to lie at the root of successful co-operation and at the root of effective co-ordination.

How can co-operation and co-ordination be best achieved? Some scholars have always taken the trouble to peer over the barriers that separate their own studies from the studies of others. Specialization is increasing the height of the barriers, but no doubt there will always be scholars big enough to see over them. Anything that encourages specialists to improve their general equipment without impairing their special equipment deserves to be fostered. It might encourage co-ordination, for example, if Dark Age studies were introduced into the universities as a separate composite subject for attention. Or a stronger public interest might follow the creation of a 'Council for Dark Age Studies' on the lines of, but with wider scope than, the Council for British Archaeology.

It would be possible to create a university department devoted to the study of the Dark Ages in Britain, but it would not be easy to staff it with men who were not mere historians or mere archaeologists or mere philologists. It would not be easy, but it should not be quite impossible. As a teaching department, however, it would be a



## PROBLEMS AND POLICIES

doubtful asset. Courses and examinations do not themselves produce historians, archaeologists and philologists, and a new course which set out to produce a mixture of the three would probably produce nothing. To be effective it would have to be a post-graduate course for students who had spent a dozen years or more familiarizing themselves with the material and processes of its constituent parts. And for these students it would not be necessary. As an undergraduate course it would be no more than the softest of soft options. The department would have little value as an instrument for training the young and it would have less as an instrument for instilling into specialists an understanding of the difficulties and methods of other specialists.

Active research alone can produce that mutual understanding. It seems reasonable to demand that the historian who aspires to use archaeological or linguistic evidence should first undergo an appropriate training. That training will not be found in books and in lectures; it is obtainable only in the trench, the museum and the library, in grappling with a problem on the other man's ground and with the other man's weapons. Admittedly this training cannot be obtained in one year or in two, and admittedly he who pursues such a policy will still find himself dependent upon the specialist. But he will add to his own equipment, he will be able at least to ask intelligent questions, and when he attempts to co-ordinate evidence he will stand on safer ground than the specialist who has never wandered from his own field. Finally, it is possible that, backed by his own specialized techniques, he will add to the techniques of the other specialists with whom he has worked.

Such wandering specialists will naturally make their own contacts and their own arrangements. They have always done so, and they need no council or institute or department to give them a lead. But an organization of some kind would assist them in their efforts to co-operate with each other, and by its very existence it would stimulate interest in Dark Age studies. An institute or department devoted specifically to the many-sided problems of the Dark Ages would be more than a centre for discussion and a pool for information. It could attempt much that individual scholars, however enthusiastic, cannot attempt until they are drawn together within a single organization. A research institute staffed by trained historians, archaeologists and philologists, all bound together by a common interest in Dark Age problems and by a common experience of the different lines of approach to them, could formulate a policy and could direct research activity into its most useful channels. The trained archaeologist would devote some of his time to accumulating and examining archaeological evidence with specific *historical* problems in mind. The historian would devote himself not only to using archaeological evidence for historical purposes but also to accumulating historical evidence for a planned attack upon *archaeological* problems. It has never yet been done. The philologist would bring his material and his methods to bear not only upon linguistic problems but also upon historical and archaeological problems. These few men, inevitably few, could go far towards co-ordinating different and apparently divergent accumulations of evidence. Each would be a specialist in his own field but—this is important—each would have had experience in other fields.

It could be done. Three or six men could do much if they were brought together in this way, and they could train assistants at the same time. The historian who had accumulated evidence for the identification of a lost site would have expert advice at his elbow; he could have his linguistic evidence examined and he could have his theory put to the final test of the spade by men interested in the same problems. To-day if the historian cannot fire the interest of a busy excavator he must either mutilate the site himself (for the spade alone can prove or disprove his theory) or he must wait until

some Bronze Age specialist accidentally mutilates it for him. In either case the vital evidence may well be lost forever. The archaeologist, in his turn, could call for precise historical information instead of having to accumulate it for himself from out-of-date textbooks. An institute could secure planned and permanent co-operation, and that surely would be more fruitful than the casual mutual assistance upon which we depend to-day. Moreover the historian would find that his experience as a 'dirt archaeologist' gave him incalculable advantages over historians who had seen excavations only from well polished shoes. And the historian who managed to see place-names through the eyes of a philologist would use them the more effectively. Likewise the archaeologist and the philologist would gain by close association with the historian, his material, his problems and his methods.

Until such an institute provides a centre for Dark Age studies all scholars who are interested in these problems would be well advised to assist at excavations, to try to see place-names as philological phenomena, and to consider carefully the historian's pre-occupation with questions of source, bias, reliability, etc. The aim of each should be to improve his own equipment, not to compete with other specialists.

A Council for Dark Age Studies, created on the model of the Council for British Archaeology, is not the same thing as a research institute. It would have a different task, the stimulation and organization of a wide and interested public. Archaeologists can teach other scholars much about salesmanship. To-day the public are the patrons; they provide, directly or indirectly, equipment, departments, funds and, if properly encouraged, lavish journals. They have a right to be taken into the confidence of scholars, they are anxious to be interested, and they are often willing to assist. It is the duty of scholars to produce readable summaries of their work, and archaeologists have shown that it pays. Separate reports surveying the non-archaeological problems of the Dark Ages, followed by a combined co-ordinating report, and all written in simple lucid prose, have been envisaged above. They would be of limited value to scholars actively engaged in research, but they would dissipate the mists of legend, ancient and modern, with their shades of long-haired Druids flitting whitely through scenes of midnight slaughter and mid-day revelry. The mists would be blown away, and readers would see the Dark Ages as a period of fascinating and interlocking problems, a period which calls for the best of modern techniques and which offers great scope for those who are interested in it. They would find from these reports that modern methods are more satisfying than ancient legends, and they would find in them clues which would enable them to reject many of the modern legends which pass for history. And no doubt they would derive considerable enjoyment from the spectacle of specialists entangled in academic webs of their own weaving.



# The taking of Le Krak des Chevaliers in 1271

by D. J. CATHCART KING

THE great Syrian fortress of Le Krak des Chevaliers, the best known, as it is on the whole the finest and best preserved, of the Crusader castles, has recently been made the subject of a brilliant and exhaustive study by M. Paul Deschamps<sup>(1)</sup>; taken in conjunction with earlier notices by Rey, Van Berchem, T. E. Lawrence and others, this has left archaeologists in an exceptionally favourable position with regard to their knowledge of this castle. A few objections on points of detail might be raised to Deschamps' conclusions, but it appears to me that there is only one important question on which further clarification of our existing knowledge is needed: the reconstruction of the siege and capture of the Krak by the great Mamluk Sultan of Egypt, Bybars, in April 1271. The accounts given by the Arab historians of the period are not perfectly clear, and the explanations offered by Van Berchem and Deschamps do not appear to be the best possible, in view of the evidence of the fabric itself. In order to obtain a sounder idea of what occurred in the siege, it will first be necessary to consider the buildings and their history in brief.

Le Krak des Chevaliers stands on a foreland of the bare black basalt hills about the Homs Gap—the important pass, between the Lebanon to the south and the Jebel Ansariyeh to the north, through which runs the road from Tripoli to Homs; it looks out over a fertile and well-watered plain to the east. A position of great strategic possibilities, it was occupied even before the Crusades by a Kurdish garrison, from which it took its earlier name of Hosn el Akrad—Stronghold of the Kurds<sup>(2)</sup>. This garrison was ejected by Raymond de Saint-Gilles in 1099, but the castle was only finally occupied by the Christians in 1110, when Tancred, Prince of Antioch, took it for the benefit of his ward Pons of Tripoli. In 1142 Raymond I, Count of Tripoli, gave it to the Knights Hospitallers, in whose hands it remained until its final loss in 1271. They made it the centre of their great strategic position, the network of castles and towers covering Tripoli from attack from the east. They also entirely remodelled it. The castle which we see to-day is a concentric fortress, one, that is to say, in which there are two lines of defence, the inner—which at the Krak is of a highly original and extremely formidable character—being built close up to the outer, which is overlooked and commanded by it. The enclosure of this inner enceinte is a great mass of vaulted undercrofts, enclosing a dwarfed and constricted courtyard. The castle of the eleventh century was a very different structure; its remains, still incorporated in the present buildings, are those of a single-ward castle, covering approximately the same area as the existing inner enclosure. There was a gateway set between two square towers (H, FIG. 1); this still remains, as do the postern-tower P and the Chapel, whose projecting apse served as a tower; these defended it on the north and east; on the other two sides there were probably four square towers. The walls were lined inside with a continuous ring of vaulted buildings. The position of the castle was a strong one; on three sides the ground fell in headlong slopes; the eastern slope, up which the approach from the plain zigzagged,

<sup>1</sup> Le Crac des Chevaliers (Les Châteaux des Croisés en Terre Sainte) Paris, 1934.

<sup>2</sup> Hence the curiously pleonastic modern Arabic name Qalaat el Hosn (Stronghold Castle).

began directly from the foot of the walls, but there was a distinct level space on the north and west between the walls and the scarp ; on the west at least this was partly occupied by a ditch. The south side was the weakest ; here the ground rose in front of the defences, not indeed steeply, but very perceptibly ; there was no doubt a formidable ditch on this side.

Strong though this castle was—and the great Saladin, at the height of his fortunes, had misliked its appearance too much to attempt its capture—it was not strong enough for the ambitious rôle in which the Hospitallers had cast it, and at some date close to the opening of the 13th century it was very thoroughly reconstructed. On the north and east they left the existing defences unaltered, but the weaker south and west fronts were remodelled, the small square towers being replaced by large round ones, and the whole line of defence encased in a vast inclined talus, out of whose slopes the great towers rise majestically ; carried out in first-class masonry, this defence is extremely beautiful, and also very strong indeed. On the south, where the defences were advanced considerably in front of the older line, the talus rises from the bottom of the original ditch, now represented by the great cistern, whose lofty counterscarp partly conceals the immense slope of the walls. On this side there are three enormous round towers—I, J and K, set close together, forming what is probably the finest medieval line of defence anywhere ; they are of immense height and solidity, and are linked by two high terraces, supported on two storeys of vaults ; these terraces are connected by a wide crenellated ledge, passing round the face of tower J ; on the western terrace there stands a large vaulted structure of post-Crusader workmanship, which seems to have replaced an older construction. The upper room of tower K is a pleasant apartment, having a large window with floral enrichment ; this was evidently where the constable of the castle, or the Grand Master on his visits to the Krak, would be accommodated. The two upper chambers of tower J are lighted respectively by one and two fair-sized windows of two lights with ornamented heads ; here, and in the upper chamber of tower I (whose windows have been robbed) we would naturally expect the quarters of the Knights to have been ; there were 60 of them in garrison in 1255 (3) and this would be quite adequate accommodation for them—ample if we include the basement of tower J, a rather cheerless, unlighted room, and the building between towers J and K. Certain precautions were taken to protect this set of quarters ; across the back of tower J and the terrace between it and tower I a gap about ten feet wide was left in the lower vaults (4), leaving a protective drop to ground-level, which Deschamps calls a *saut-de-loup* ; this gap was crossed by a bridge—possibly a movable one—at the head of which there seems to have been a door. These good quarters and the measure of protection given them emboldened M. Enlart to allude to the three great towers as the *Donjon* of the place, and Deschamps repeats this description without apparent question (5), despite Rey's warning that the strong defensive works used by the Hospitallers are not to be confused with the European *donjon* or keep (6)—that is to say, are not the normal strong work, capable of independent defence after the rest of the castle had been captured. A little reflection will show how profoundly correct Rey's statement is for the Krak : none of the three great towers—much less the three together—can be regarded as independently defensible ; towers I and K have no stairs, and there is no communication between their floors except by way of the outside world ; on the

<sup>3</sup> Bull of Alexander IV, quoted Deschamps, p. 130.

<sup>4</sup> Its position on the plan is marked by almost the only Mamluk work in the inner ward, the Muslim owners having closed the gap.

<sup>5</sup> *op. cit.*, p. 205.

<sup>6</sup> *Architecture Militaire des Croisés*, p. 15.





a. VERTICAL PHOTO. OF RELIEF-MODEL OF PLAIN OF N. APULIA, BETWEEN THE APENNINES AND THE GARGANO PROMONTORY  
Scale about 1 in. to 16 miles

A=Arpi: AS=Ascoli Satriano (Ausculum): C=Canosa (Canusium): F=Foggia: L=Lucera (Luceria):  
O=Ordona (Herdonia): S=Salpi (Salapia): SI=Sipontum: T=Troja (Aecae): TA=Tea-num Apulum  
1=Plates iv and v: 2=Plate vi: 3=Coppa Navigata



b. OBLIQUE PHOTO. OF SAME, LOOKING S. OVER THE PLAIN AND DOWN THE COAST TOWARDS BARI. WITH SPOT-HEIGHTS ON THE MURGE (1300 ft.), THE APENNINES (3750 ft.) AND THE GARGANO (3430 ft.)

PLATE II



A VILLAGE (240 YARDS ACROSS) WITH 14 HUT-ENCLOSURES OF NEOLITHIC TYPE; REVEALED BY CROP-MARKS  $3\frac{1}{2}$  MILES N.W. OF LUCERA

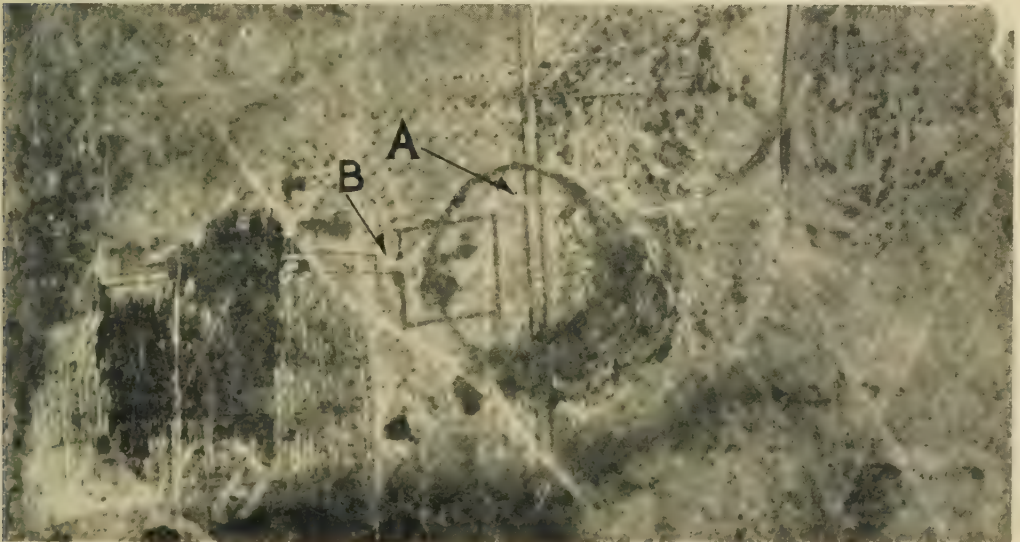
Scale 1 : 3500



PLATE III



A



B

THE UPPER PHOTO. *a* (Scale 1 : 3500) AND THE LOWER *b* (Scale 1 : 3000) BOTH SHOW RECTANGULAR ROMAN FARM ENCLOSURES N.E. OF LUCERA, WITH ADJACENT VINEYARD PATTERNS, CONNECTED TO CENTURIATED ROADS. THEY ARE SUPERIMPOSED ON EARLIER CIRCULAR ENCLOSURES. ALL ARE REVEALED BY GRASS-MARKS AND CROP-MARKS

PLATE IV



CROP-MARKS NEAR CASA SCOPPA

A=intersection of two centuriated Roman roads (*limites*).  
B= the pattern made by cultivation-pits in a Roman vineyard or orchard.  
E=a modern olive-grove for comparison.  
C, D=other crop-marks.





A



B

- a. THE INTERSECTION OF TWO CENTURIATED ROMAN ROADS (A), AND THE PARALLEL TRENCHES OF A ROMAN VINEYARD WITH A PATH THROUGH IT (B), ALL SHOWING AS CROP-MARKS; 4 MILES S. OF FOGGIA. Scale 1 : 3500.
- b. A LARGE EARTHWORK (A) OF MEDIEVAL TYPE AT TORRIANE DEL CASONE, 13 MILES N.N.W. OF FOGGIA. THE OUTER HORNWORK (B) AND THE TRACKWAYS (C TO G) WITH SMALL FIELDS BETWEEN THEM APPEAR AS CROP-MARKS

PLATE VI



A PROBABLE *PALACIUM* OF THE EMPEROR FREDERICK II AT SAN LORENZO,  $3\frac{1}{2}$  MILES S. OF FOGGIA ; REPRESENTED BY A MASSIVE EARTHWORK OF MEDIEVAL TYPE, WITH 3 ENCLOSURES (A, B, C) WITH TRACES OF ROADS APPROACHING THEM (D to G), AND FIELDS BETWEEN THEM. X Y AND Z ARE MODERN FEATURES

Scale about 1 : 8200



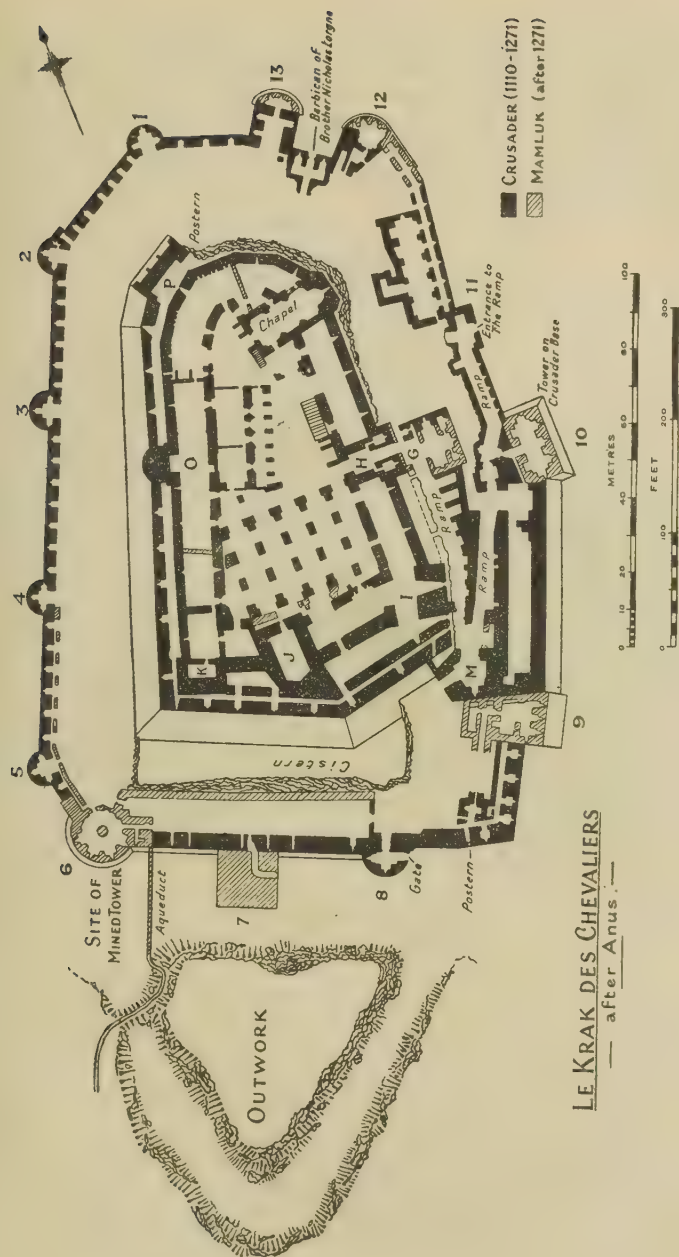


FIG. 1

# LE KRAK DES CHEVALIERS — after Anus —

## ANTIQUITY

lower levels these floors are quite defenceless (v. plan, especially for tower I) ; tower J is at least a self-contained structure, but its defensive possibilities were slight ; with two unfortified doors, both perfectly accessible, three large windows overlooking the courtyard, and not a single loophole directed towards the inside of the ward ; standing between tower K, which is its own height, and tower I, which is very little lower, and with covered access to its base on both sides available to the enemy, it could offer little resistance once the rest of the ward had fallen.



FIG. 2. LE KRAK DES CHEVALIERS: SOUTH FACE OF THE INNER WARD ; TOWERS I (in background) AND J, SHOWING THE SENTRY-WALK ACROSS THE FRONT OF THE LATTER  
(Since the date of this drawing, the parapets have suffered extensive damage)

It may be asked at this point what was the purpose of the elaborate protection of the *saut-de-loup* ; and it appears to me quite certain that the significance of this *area* (in the 19th century domestic sense) was not strictly defensive ; it existed for reasons of internal security. The garrison of the Krak included some dubious elements, particularly among the Turcoples, or mercenary light-horse ; and while nothing in the nature of a mutiny *en masse* of the garrison was to be expected, there was always a danger of a *coup de main*, probably taking the form of a night attack and the assassination of the constable or the Grand Master in bed. Viewed in this light, the arrangements are excellent. The 'area' could be crossed easily with a ladder, but the mere possession of a ladder after dark would arouse suspicions, and it would be impossible to reach the terrace without attracting the notice of a sentry. In the same light, other arrangements of no real defensive significance are seen to have placed very grave obstacles in the way of treachery : the absence of stairs in two of the towers, and their disposition in tower J so that to reach the terrace it was necessary to pass through each room in turn, beginning with the less



comfortable rooms, which inevitably would be occupied by the junior knights—unrewarding, and most dangerous, subjects for assassination; finally, the arrangement of the great set-off on the face of tower J—round which lay the only access to the commander's quarters in tower K—with a door at each end so as to form a sentry-walk: all these precautions, useless against an enemy in overwhelming force, effectively disarmed internal treason. It has been found necessary to insist here on the fact that the great towers formed no sort of last refuge in a formal defence for the better explanation of the siege, as will appear below.

At the same time as the inner defences were remodelled, an exterior line of defence was added, carrying the perimeter of the fortress nearer to the top of the escarpment on the north and west; on the east the new line was inevitably carried down the slope, where it enclosed a limb of the zig-zag approach; on the south side it was advanced uphill on to a more unfavourable position, the ground sloping upwards rather sharply, so that the counterscarp of the ditch is almost level with the rampart-walk of the ward (7). This ditch, which is cut in the rock, and is about 30 feet deep on the uphill side, is a most formidable obstacle.

Later in the 13th century the castle was further reconstructed; on the east a series of great vaulted buildings was erected, and the line of defence further advanced down the slope, enclosing a second limb of the approach, so as to form in its entirety the famous main entrance, the Great Ramp. Beginning at a gate in the face of tower 11, it forms a dark vaulted tunnel about 150 yards in length, with open sections at intervals, and a hairpin corner in the middle; no more unappetising entrance could be offered to an assailant, for it looks the sort of place where death might lurk at every step. Nor is this appearance delusive; the outer gate in tower 11 is weak, and the adjoining curtain is so thin that it appears that this part of the defence was a mere barbican; but the assailant would soon come up against a serious obstacle—the formidable double gate behind tower 10, the entrance to the outer ward. Beyond here the way divides, one road curving to the right and entering the open space between the lines of defence, the other continuing upwards to the elbow of the ramp. Here a strong gate (M) defends the ramp against attack from the south; at the elbow itself are the jambs of another gate, and the remains of a number of auxiliary defences of a perverse ingenuity. Behind this the ramp led up to the inner gate H and its fore-tower G. There is no gate in the fore-tower, nor do I think there ever was one, even before the rebuilding of this structure by the Mamluks. The gate H, however, was strongly fortified, with doors, a portcullis, and a large open machicolation in front. The whole length of the ramp is lined with openings in the crown of the vault; no doubt these served to admit light and air, but the careful tactical siting of some of them shows only too well that they were meant to let through more solid things than light.

To this later period belong the other outer gates, of which the small, well-defended entrance between towers 12 and 13 is dated to a time close to the middle of the 13th century by the inscription "*Au tens de Fre(re) Niciole Lorne fu fete ceste barbacane*"—"this barbican was built in the time of Brother Nicholas Lorgne" (8). On the south there is a gate in the flank of tower 8—one of the original towers of the outer line—which Deschamps considers to be an insertion (convincingly, because its curious and inconvenient arrangement, with the gates in the flank and the portcullis in the gorge of the

<sup>7</sup> The parapet will have been of two storeys, in the Eastern fashion; this must have given a reasonable command over the counterscarp.

<sup>8</sup> Constable of Margat, 1250-4, and of the Krak presumably soon afterwards.

tower (9), could scarcely have emanated from the immensely competent architects of this line) and a short distance to the east of it a postern, opening in the curious salient between towers 8 and 9. Both these entrances were no doubt built to give access to the big triangular outwork on the south, which was evidently constructed about this time, to judge by the amount of black basalt rubble used in the construction of the later part of the defences; this evidently came from its rock-cut ditches. That the builders planned to give this outwork walls and towers seems probable; what defences it actually possessed at the time of the siege cannot at present be ascertained. Rey (10) suggests that it was palisaded, but timber is scarce locally; even in treeless Syria the Homs Gap is an exceptionally treeless region. It may have been given some provisional defence in the form of dry walling or light masonry. There is certainly no trace of any such work now, but the silted and encumbered condition of the two strong rock-cut lateral ditches suggests that it may have been cast down the scarp. It seems unlikely that the Hospitallers neglected to defend such an advantageously sited outwork, even though its walls may not have been sufficiently advanced to warrant a prolonged defence.

Before leaving the triangular outwork there are two last questions to be considered, both involving gradients. In the first place, the pronounced northward slope of the ground is masked by the high counterscarp and the outer curtain behind it, so that the full strength of the inner defences is concealed from view. The second fact is the manner in which the side ditches come into the main cross-ditch in front of the outer ward; for whereas the western ditch debouches on level ground, the eastern side of the castle has been twice enlarged downhill, as has been mentioned already, and as a result the eastern ditch opens on a steep slope.

Finally it appears that the Krak possessed a substantial walled suburb or *burgus*, (11) of which there appear to be no remains whatever.

Coming now to the narrative of the siege, I propose to examine the evidence, which is principally based on three Arab writers: Ibn Shaddad, Ibn Furat and Nuwairi (12); of these, Ibn Shaddad was a contemporary, but not an eye-witness, having joined the army three weeks after the siege; the other two wrote half-a-century later. For this reason, the account of Ibn Shaddad will be used here as the controlling narrative, and his dates will be accepted for the purpose of this article, though I cannot pretend to imagine, in face of the feats of the national Press during the recent war, that there is of necessity any advantage in accepting a contemporary account. The stages of the siege are narrated as follows:

(A) *3rd March 1271*. Ibn Shaddad describes the Sultan as camping before the Krak and setting his mangonels in battery on this date: 'Now the castle had three lines of walls and three *bashuriyas* (a term whose meaning will be dealt with later). He pressed on the siege to the destruction of the ramparts on the 24th of the same month (13) (8th March).

Ibn Furat makes the siege open on the 21st February, and Marino Sanudo and the Christian authorities generally on the 18th.

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<sup>9</sup> So that the fighting-stage in the basement would be closed when the portcullis was down, and that on the upper floor when it was up!

<sup>10</sup> *op. cit.*, p. 46.

<sup>11</sup> Deschamps *op. cit.*, p. 120; Rey, *op. cit.*, p. 40.

<sup>12</sup> The excerpts from these writers are taken from the French versions quoted at length by Rey (*Arch. Mil.*, pp. 65-7), and Deschamps (*Le Crac des Chevaliers*, pp. 132-4).

<sup>13</sup> The Moslem month of Rejeb.



(B) 5th March. Ibn Shaddad : ' One of the *bashuriyas* was taken '.

Ibn Furat : ' On the 20th (13) (4th March) the " faubourgs " of the castle were taken '. Nuwairi says the same.

Now follows a delay in the siege ; Ibn Furat and Nuwairi merely mention that the engines were being set up, but Ibn Shaddad says : ' operations were held up by incessant rains '. It appears that the siege was not pressed during this period.

(C) 21st March. (14) Ibn Shaddad : ' The second *bashuriya*, called Haddadiya or Blacksmith's, was taken '.

Ibn Furat : ' The *bashuriya* was taken by storm. A place was made for the Sultan to shoot from '.

(D) 29th March. Ibn Shaddad : ' (the third *bashuriya*) was taken by the miners of Malik Sa'id . . . Then the troops stormed their way into the castle, massacred the Hospitallers, took prisoner the mountaineers, but let the villagers go, to keep up cultivation in the country '.

Ibn Furat : ' On the 16th of Shaban (30th March) one of the towers was broken ; the Mussulmans made an attack, got into the castle and took it. The Franks fell back upon " the summit of the hill or of the castle (15) " . . . Engines were brought up into the fortress and trained on the hill '.

(E) 8th April 1271. The Krak surrendered on this date, the garrison obtaining terms under which their lives and liberty were spared, and they were given a safe-conduct to Tripoli. Ibn Furat and Nuwairi mention that the Sultan sent in a forged letter, purporting to come from the Grand Master at Tripoli, giving permission for the garrison to surrender. Apart from the statement by Ibn Furat, quoted above, that mangonels were brought up, there seems to be no mention of any siege activity in these last ten days.

Van Berchem, and Deschamps after him, visualize an attack directed from the east, where the slope is said to be less formidable than on the north and west. The allusion to the Castle as having three lines of walls is said to support this. Van Berchem's interpretation of the chroniclers is an attack up the Ramp, beginning with a breach in the curtain 10-11, and ending in the defence being driven into the three great towers. As regards the breach, Deschamps shows that the curtain 10-11 was only refaced by the Arabs and not rebuilt (16). His own idea of the progress of the siege is more cautious, and is worth quoting in full :

' Ibn Shaddad, who gives the closest account of the siege, writes : " the castle had three lines of wall and three *bashuras* (barbicans) ". From the fact that the eye-witness from whom Ibn Shaddad no doubt obtained this information saw three walls, it can be inferred that the main attack was made against the East front, that is to say, from the side of the bent ramp, whose walls could make up, as it were, three ramparts.

' The *bashura* or barbican taken on the 5th of March—which Sobernheim translates as " tower of entry ", must be either tower 11, or the barbican 12-13.

<sup>13</sup> The Moslem month of Rejeb.

<sup>14</sup> This appears to be the correct date, but Deschamps (p. 133) for some unaccountable reason takes the 15th for the date, on the apparent authority of Ibn Shaddad, though the latter in fact gives the 21st.

<sup>15</sup> This expression, which occurs in Rey, I take to be a translation of the word *gulla*.

<sup>16</sup> p. 303.

The second barbican taken on the 15th (17) (the tower of entry of the second enceinte according to Sobernheim) could be the gate at the entrance to the second element of the Ramp, the work G, or the postern of the tower P.

'Finally, the third barbican taken on the 29th March, in the attack on which the miners of Malik as Saïd took part, will be the set of defences placed at the opening of the ramp on the courtyard (the tower H).

'Then the Moslem soldiers stormed into the courtyard, where a certain number of knights were killed fighting. The last defenders took refuge in the great towers of the south . . . Then it is the 8th April, the surrender'.

Now any interpretation of the accounts of the siege must depend to a great extent on the translation of two Arabic words: *qulla*, a pleasantly vague word, meaning a tower, a peak, a small castle, or even a very large one (18); and *bashura* or *bashuriya*, which historians mostly translate as 'gateway' or 'barbican' in the narrative of this siege. Rey, however, makes it mean an outwork or advanced line of defence, and this is the translation given to it in other contexts (19). In fact, the most exact interpretation of the word is 'a bent entrance' (e.g. tower 8) (20), and 'barbican', in the loose medieval sense in which Brother Nicholas Lorgne's contemporaries used it, is a reasonable enough rendering. Whether Ibn Shaddad used it precisely, and if so, whether he knew exactly what he was talking about, is another matter altogether, and it might prove an unprofitable labour to attempt a reconstruction of the siege so as to involve the capture of three gates and three only, preferably with bent entrances, especially when we recall that it is quite possible that the gates of the *burgus* and of the triangular outwork may be included in the three *bashuriyabs* mentioned. It may, however, be worth pointing out that a forcing of the Great Ramp cannot be reconciled with this description; there were at least four gates, one of them double, in the way of this line of attack.

There are other drawbacks to the theory that the besiegers forced the Ramp: it seems curious that Bybars should have attacked on the east, in many ways the strongest side of the fortress; but the mere difficulty of the operation, and the *obvious* difficulty of it—the very appearance of the Ramp is alarming—make it unlikely that any assailant would willingly attempt it. Nor am I much impressed by the argument that the attack must have come from the East, because Ibn Shaddad mentions three lines of walls, and on this side the fortress presents the illusory appearance of having three lines; in fact it merely appears to be a jumble of towers; if on the other hand there was so much as a breastwork round the triangular outwork the castle would certainly have three lines of walls when viewed from the *south*.

But this is not the weakest part of Deschamps' and Van Berchem's case. Let us consider the three stages of the development of Bybars' attack, stages B, C and D above, corresponding to the taking of Ibn Shaddad's three *bashuriyabs*: Stage B was reached almost at once—only two days after the opening of the siege—and is strongly suspect of involving only a dependency of the castle, probably the *burgus*; on Stage C we have less information, though Ibn Furat's allusion to a place being made for the Sultan to shoot from seems to suggest that the position which had just been taken was a fairly commanding one. But we have one most important piece of information about Stage D: it was achieved by mining; Ibn Shaddad mentions miners, and Ibn Furat states that a tower

<sup>17</sup> Vide supra, note on this date.

<sup>18</sup> The gate of the Citadel of Cairo is called Bab Qulla.

<sup>19</sup> Deschamps, p. 120; Rey, op. cit., p. 151.

<sup>20</sup> I am indebted to Professor K. A. C. Creswell for this fact.



was broken. We can deduce other facts: Stage D involved the fall of a major work, not just that of a single tower, for Ibn Furat calls it 'the castle', and Ibn Shaddad mentions a heavy slaughter and the taking of prisoners, including the local villagers. It would be tempting to follow Deschamps and see in this the capture of the inner ward, thus identifying the *qulla*, into which the Knights retreated, with the three great towers. But this interpretation at once raises difficulties, for whatever the *qulla* was, it was obviously a very strong position. The Knights held out there for ten days, and only surrendered on favourable terms, and after receiving a forged permission to yield. It may be objected that a good deal of this time may have passed in negotiations, but it is obvious that only a strongly-placed defence would be allowed to waste the besiegers' time and obtain good terms at the end of it. The Circassian Mamluk Bybars was a fine fighter—bold, resolute and resourceful, but treacherous, cruel and fanatical; such a man would hardly grant an extended parley and generous terms to a garrison who were at his mercy; and if the *qulla* is to be identified with the three great towers, they would certainly be at his mercy, for as we have seen, they would have had no adequate defence.

Moreover, if the inner ward fell, if fell by mining; where then are the traces of the mine? The excellent plans of M. François Anus, on which the plan to this article is based, show no 'Arab' work in the inner ward at ground level except a very few interior features—partition walls and the like. The fore-tower G has been rebuilt, but even if this contained a gate, its fall would not have involved that of the inner ward, nor of any place sufficiently large to accommodate the number of people that Ibn Shaddad mentions. In short, it is clear that Stage D was the fall of the *outer* line, not the inner, and that the *qulla* is the inner ward.

We have now progressed some distance, but there remains a final question to be settled: where the mine was sprung. Now the outer ward, unlike the inner, has undergone a great deal of reconstruction by its Mohammedan owners. Much of this consisted of refacing, but there are a number of points at which the whole wall is the work of the Mamluk owners: towers 6, 7, 9, 10, the face of 12 and a length of the curtain adjoining, and the curved portion of 13. Of these six, three can be dismissed at once: tower 7, an immense square tower built by Sultan Qalawun (21) stands in front of a Crusader curtain-wall which has been refaced, but is otherwise complete; tower 10 occupies an undisturbed Crusader plinth, and tower 13 is still very much as its Christian builders left it; the Saracen work consists merely in a bow-front applied to the tower to change it from the rectangular to a form approximating to the round. A similar change was made in the companion tower 12, but here the alteration was more extensive, involving a considerable piece of the curtain. This is, however, somewhat reduced in length if one considers the Crusader structures remaining intact behind it, and the total width of the breach in the Frankish work is no more than 40 feet. The narrow and rather ineffective tower 9 is even less likely; for one thing it seems to have had no Crusader predecessor, for another its whole width, from the battered plinth of the great vaulted building on the north, which partly underlies it, to the curtain between it and the southeast angle, is only 36 feet; this is a perfectly possible figure for the bottom of a breach, but the gap is little or no wider at the top of the very lofty southern curtain, which could hardly be the case if there had been a violent structural collapse. Evidently the curtain was carefully cut back by the builders to make room for the tower.

This leaves tower 6, and here the gap is wide—fully 75 feet across—and includes not only the tower, but a short length of the adjoining curtains on each side. Finally, it

<sup>21</sup> A very massive structure, its base being a solid cube of masonry of side approximately 50 feet.

stands at the most readily accessible point of the whole enceinte. It must be remembered that mining operations in the Middle Ages were very seldom conducted by means of a long subterranean gallery, as they were in later periods; nor is it likely that a mine of this exceptional type would be attempted in the hard volcanic rock underlying the Krak. If it were attempted, it is improbable that it could have been brought to success in the very limited time of the siege in question here. The normal practice was to work at the very foot of the wall under cover of the strong movable shed variously called a 'cat' or a 'sow'. As the natural reaction of the defence would be to drop upon this engine all the heaviest and most harmful objects they could collect, its construction had to be massive, and hence its weight was considerable; and this weight had to be man-handled to the foot of the walls. It would have been almost impossible to do this up the violent slopes on three sides of the Krak, while most of the south side is covered by the great ditch, whose counterscarp would be the principal obstacle, being more or less vertical, and 30 feet high. To get a heavy 'cat' down this, even without the attentions of the besieged, would be a tricky and dangerous business. It followed that the best points of attack would be the southeast and southwest angles of the castle, by the way of the lateral ditches of the outwork. The eastern ditch opened on a sharp slope, as has been mentioned; the western on fairly level ground opposite a tower—a more vulnerable point than the curious salient on the east; this, then, must be the position of Bybars' mine.

We can now proceed to give an amended account of the siege and capture of Le Krak des Chevaliers: Bybars himself appeared before the fortress on the 3rd of March 1271; to judge by the earlier dates mentioned by some of the chroniclers, it may have been under observation, or even blockade, for some days previous to this. Two days later the *burgus* was taken, and on the 21st, after sixteen days of rain and hard work, setting up the mangonels on the plateau to the south of the castle, the Sultan's men rushed the triangular outwork, from a position on which Bybars was able to follow his old army trade of crossbowman. Next, the outer ward was attacked, and the southwestern tower was mined and collapsed on the 29th. Then the Moslems stormed the outer ward, which was full of the local peasantry (a practice recommended by Procopius), and slew the few members of the garrison there; these last were doubtless *enfants perdus*, with the gates shut at their backs. And now the victors found themselves facing the inner ward across a swimming gulf; the northward slope of the ground has several times been mentioned in the course of this article, together with its effect of making the three great towers of the second line crouch low behind the defences in front of them, as if to conceal how frightful they really are. But now the whole awe-inspiring spectacle was revealed. It may be said here that Deschamps' photographs, excellent though they are, are quite inadequate to convey the full impression of this incredible line of defence; it is not, in fact, a possible subject for a camera.

The Sultan was now in the dismal position of one who has forced three lines of defence, and finds the fourth impregnable.

But al-Malik az-Zahir Rukn ad-Dunya wad-Din Abu'l-Fath Bybars had not risen from a slave-soldier to a victorious Sultan for nothing; reflecting that the Knights were probably even more downhearted than himself, he tried his luck with a forged letter, in the reasonable hope that they would be unwilling to scrutinize it too closely. His resource and treachery were rewarded; the Knights asked for quarter, and were granted it, and on the 8th day of April 1271, the 'key of Christendom' passed into the hands of Sultan Bybars. Not till the war of 1914-18 was it to be in Christian hands again.



# Kingship and the Gods: a review\*

by M. E. L. MALLOWAN

EVERY schoolboy knows that King Rimush of Agade was struck down in a Palace revolt—some say pierced to death by sharp bodkins, others that his skull was smashed by heavy stone tablets. (1) And it is now almost common knowledge that Bur-Sin died of a pinched shoe—it may have been following on a septic foot contracted at Eridu, when he was pacing the sand in the precincts of his unfinished Ziggurat. No less undignified was the death of Sin-iddinam of Larsa, crushed to death in his Palace by the fall of a staircase. But who ever heard of a Pharaoh so ludicrously stricken? (2) There lies the difference between the Egyptian and Mesopotamian concept of monarchy. The Pharaoh was a god and the son of a-god, made in the image and likeness of Horus, son of Osiris: it is true that he was mortal, as was more than one of the ancient gods, but his mortality lay lightly upon him because when the time came for his end upon earth he was predestined to rebirth as Osiris. In the Tigris-Euphrates valley a king, whatever else he might be, was pre-eminently a man, even if only a little lower than the angels, and even the mighty Sargonid Kings of Assyria, could on occasion receive and accept a rebuke from their subjects. (3) Indeed the Assyrian kings, whom we are accustomed to think of as arrogant warriors, were no less the slaves of the society they served than the humblest members of their realm. From records of the first millennium B.C. we hear of an Assyrian ruler being bidden to commit on himself a painful act of depilation: on another occasion after fasting to the point of inanition he was made to persevere yet further by the relentless priest who was responsible for his abstinence; at other times he had to endure an enforced solitude, segregated like a leper in a reed hut—an act of contrition and purification on behalf of his realm. It is true that the Pharaoh's lot was often hardly any happier, for he too was hemmed in by every kind of *tabu*, and his engagement and duties book, if strictly followed, would have kept him busy for every moment of his pompous life. But at least the Pharaoh might enjoy the calm serenity that comes to those who believe that their translation to the stars is inevitable. In sum the Egyptians were optimists, for experience had taught them that the sun would not fail, that the river would rise again and that the harvests were generally good. Mesopotamian pessimism on the other hand may well have been induced by the stark uncertainty of their climate; by constant fear of invasion from the hills and the ever-recurring threats, natural and preternatural, to the fabric of their society. Perhaps the Egyptian strain of happiness owed something to a negroid admixture within their race, (4) and conversely

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\* *Kingship and the Gods*: A study of Ancient Near Eastern Religion as the Integration of Society and Nature, by Henri Frankfort, *The University of Chicago Press*, Chicago. 444 pp., 53 illustrations. Price 5 dollars.

<sup>1</sup> C. J. Gadd, *History and Monuments of Ur*, 96 ff. See also *Journal of Cuneiform Studies*, I, vol. 3, 1947, for other evidence relating to the deaths of Rimush, Bur-Sin and Sin-iddinam.

<sup>2</sup> Some of the Pharaohs suffered violent deaths, e.g. Sekenenre of the xviii Dynasty was assassinated; but there is nothing comparable in the Egyptian records to the facts quoted about these three Mesopotamian monarchs.

<sup>3</sup> R. Labat, *Le Caractère Religieux de la Royauté Assyro-Babylonienne*, 23 f.

<sup>4</sup> But a contrary opinion is held by Junker who does not believe that there was a negroid admixture. See *J.E.A.* 1921, pp. 121 ff., 'The First Appearance of Negroes in History'.

Mesopotamian gloom may have been in part the outcome of the morose and suspicious temperament which is characteristic of the Semitic and other nomads.

The theme chosen by Dr Frankfort for his study has proved to be an effective method of reanimating Egyptian and Mesopotamian religion for his readers. By focussing our interest on the king he has given us a sense of orientation in the strange and often unfamiliar realms of religious ideas. The king has turned out to be a kind of sheet-anchor alike for ancient society and for us ; he it was that gave perpetual coherence in the ordering of the state and in the tradition of religious practice. The contrast between Egypt and Mesopotamia which emerges from a study of their religious beliefs has been nicely re-emphasized by a brief epilogue on the Hebrews, whose prophets were fundamentally antipathetic to the more ancient concepts of monarchic rule.


A great variety of religious topics comes up for consideration in this book which deals in the first half with Egypt, in the second with Mesopotamia. When the curtain rises on the Egyptian stage, Menes is seen to be the hero of the first act : in him as the first representative of a united Lower and Upper Egypt we find that tendency towards Egyptian dualism which Frankfort constantly detects in Egyptian thought. Two thrones, two crowns, life and death, body and soul, the Ka as the animating principle of life, Osiris and Horus, the relation between the dead and the living monarch : all these concepts may be interpreted as evidence of a tendency to reflect on the Universe as a dualism of forces held in an unseen equilibrium. It is true that the Egyptians did not expressly formulate a dualistic philosophy such as that of Zoroastrianism ; but it is evident that they had an acute awareness of the unseen powers which were constantly at hand and mysteriously manifested to man. Thus the power of the ghosts of the king's ancestors was revealed in the guise of wolves or falcons—ghostly relics of the more widespread beliefs characteristic of the Old Stone Age : these spiritual origins have recently been vividly described in Miss Rachel Levy's book *The Gate of Horn*. Such phantasies and many others besides emerge from the shadows of the great series of Egyptian festivals which embodied the aesthetic perception of a balance between the functioning of man through the king, and the ordering of the cosmos through the gods. In the Coronation and Accession ceremonies, the Sed festival of the King's jubilee, the Transfiguration and the Mystery Play of the Succession, we find the scenic representation of Egyptian belief, moving and passionate, set in intention : yet at this distance we can but see them through a glass darkly. On all these subjects Dr Frankfort has clearly formulated his ideas and has never left us in any doubt about what he means. But much in the ancient texts is still obscure, much was left unsaid by the people ; much was thought and much more was felt : consequently we can hardly be expected to be convinced by all the conclusions of any one interpreter. For example : on the subject of Isis, Frankfort holds the view that in pre-Ptolemaic times the function of that goddess was to act as a kind of hypostasis for the king's throne (5). I cannot see that the evidence favours his theory at all strongly and there to my mind his persuasion has failed. He has however drawn an effective contrast between Hathor and Isis. Hathor who dominates the topmost register of the Narmer palette was an exceedingly ancient goddess,—surely a common heritage of predynastic African and western Asiatic belief ; her function was to act as the divine mother and she was imagined as a cow ; she betokened the Pharaoh's divine right of succession, and in her capacity as a procreative goddess symbolized a long-held belief in the link between the reproductive powers in nature and

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<sup>5</sup> It is, however, true that her name means 'seat'. None the less, as I have argued below, I believe that her powers had a wider significance.



in man. Isis, who incidentally was by no means always represented with a symbolic throne, was in Pharaonic times a comparatively neutral figure. Her presence was necessary because it was felt that Osiris had to be accompanied by a female counterpart—wife rather than mother, and as wife it followed that she was less obtrusive than the mother. For, as Frankfort has well emphasized, the Egyptian religion was essentially masculine. It was the king and Osiris who held authority over the living and the dead, a Sun-god and not a Sun-goddess who was supreme in the pantheon, and it is significant that when Hatshepsut, a queen, came to rule over Egypt, she was frequently represented in the guise of a man.

How delicate is the task of interpreting the complexities of Egyptian religious belief may be seen by comparing Frankfort's explanation of the Pyramid with that of I. E. S. Edwards, who has recently discussed this problem in his Pelican book on the Pyramids of Egypt. Frankfort accepts the thesis previously sketched by W. B. Kristensen and De Buck, that the Pyramid 'was the specific Heliopolitan form of the Primeval Hill, the Benben.' According to Frankfort, therefore, the Pyramid, while it was the Pharaoh's resting place, was also the symbol of the first land to emerge from that watery chaos which according to the Memphite theologians was the source of Ptah the Risen Land, the first evidence of creation. Such a concept incidentally was an intellectual account of Creation which ran counter to the popular belief in the Sun as the source of life. However that may be, it seems to me that I. E. S. Edwards (6) has given us a far more convincing account of the true significance of the Pyramid in relation to the Pharaoh. He has pointed out that the Pyramid is a pictorial representation in solid form of the rays of the sun shining down upon the earth; that the pyramid texts describe the king as ascending to heaven on the rays of the sun; and that *m(e)r*, the Egyptian word for a Pyramid, is capable of interpretation as a root meaning 'to ascend' prefixed by the letter 'm' conveying the meaning of place. The Pyramid would therefore mean the place of ascension, emphasizing thereby the means by which the Pharaoh in his solar aspect ascended to heaven. It would seem therefore that the true Pyramid was not the material replica of the Primeval Hill of Creation, but rather of the Sun's rays. Moreover the hieroglyph in the form of a double staircase  attached to the sense sign 'r' meaning ascent could well have been a stepped Pyramid.

When Frankfort turns from Egypt to Mesopotamia he is at pains to stress the difference between the two views of the Universe. And he expresses this by saying—and he says it repeatedly in more or less similar terms,—that the Egyptian 'could not conceive anything preceding the establishment of his static world'. His meaning is clear enough: it emphasizes the distinction between the Egyptian belief in a well balanced pattern of natural forces, and the Mesopotamian awareness that man and society were but precariously established on earth and wholly dependent on the whims of the capricious gods. But the use of the adjective 'static' to describe this Egyptian concept is not altogether happy, for static implies something immobile as well as in equilibrium. The only thing static about the ancient Egyptians were their massive monuments; their world was nervously mobile and everything was in a state of rotation, from the Sun in his bark crossing the sky by day and the underworld by night, to Osiris and the king-rainmaker or the seasonal fluctuation in the level of the Nile. The fact is that to the ancient Egyptians the world was a recurring succession of events, only pre-ordained in so far as a supreme effort was made by the divine king and the hierarchy to bring the right ordering of affairs to its expected conclusion. And when the outcome did not come up

<sup>6</sup> I. E. S. Edwards, *The Pyramids of Egypt* (Pelican Books), 234 ff.

to expectations then the Egyptians were not a little puzzled ; but like ourselves they came through dark periods by a resolute refusal to accept the facts, thereby for nearly three millennia avoiding the logical consequences of external pressure and alien ways of thought.

We have already noted at the beginning of this Review a point to which Frankfort has often drawn our attention, that the Mesopotamian king was always more human than divine ; aspire as he might to divinity, his feet were firmly planted on the ground, and he had no heavenly counterpart like the Egyptian Osiris, who was the dead king incarnate in heaven. In discussing the relationship of the Mesopotamian king to the gods Frankfort has drawn a sensitive sketch of the theological concepts, and he has shown how dangerous it is to accept the divine sign which sometimes preceded a king's name at its face value. It is true that at certain periods of Mesopotamian history there were actually cults of the king, for instance of Shulgi at Ur ; that Naram-Sin was represented as wearing the horns of divinity ; and that one passage in the cuneiform texts relates that Eannatum of Lagash was actually suckled by a goddess. But generally speaking it is clear that the expressions describing the divine parentage of Assyrian and other kings in the Tigris-Euphrates valley were figures of speech, drawing attention to the divine right of kings, rather than to physical kinship with the gods. Indeed Frankfort has acutely remarked that the Sumerians wrote that *kingship* descended from heaven : they did not say *kings*. The fact is that the divine nature of kingship has been evoked in many different places and at many different periods by monarchs and their political satellites, without implying that the king himself was divine : it was the Egyptians who were pre-eminent in stressing the actual divinity of the king's person as a physical manifestation of god.

Far more mysterious than the kingship were the divine Mesopotamian symbols ; statues, suns, moons, crown, sceptre and other regalia which, as Thureau Dangin has pointed out in the Akkadian texts, were thought of as endowed not only with life, but also with motion (7). There is a simple and touching humility in the worship of the Assyrian kings' mantle which in the absence of the king's person might be displayed in the cities of his realm to receive propitiatory prayers. How poignant a reminder that is of Christ's healing of the sick woman at Gennesaret who had but to touch the hem of his garment to be healed by the virtue that lay therein (8). That humble faith in the power within the garment of Christ the King must have been deeply rooted in a more ancient belief that the virtue of kingship rested in the king's mantle. Nor has the notion concerning the symbolic nature of the mantle yet left western Asia or Africa where the captains of pilgrim bands to Mecca, fountain head of Islam, are invested at a solemn ceremony with a gorgeous robe, the symbol of their holy mission.

In comparing the Mesopotamian with the Egyptian king it was inevitable that some discussion of their funeral rites should occur, for the disparity of practice between those two countries brings out as clearly as any other aspect of their religions the differences of concept concerning the king. The death of a Pharaoh was an event in which the whole of the Nile valley was vitally concerned, involving as it did the Succession in heaven and the dead king becoming Osiris, while on earth the living counterpart of Horus had to assume the kingship through a series of progressive ceremonies at which Egyptians from most distant parts of the country must have converged. By comparison our knowledge of what happened in Mesopotamia when a king died is meagre, though it may be inferred that Assyrian royal burials were attended with every show of pomp and circumstance ;

<sup>7</sup> F. Thureau Dangin, *Rituels Accadiens*.

<sup>8</sup> *Matthew* x, 20, 21 ; *xiv*, 34 ; *Luke* vi, 19.



but the theological significance of the Succession, so prominent in Egypt, was relatively unimportant in the Mesopotamian ceremonies. The burial of an Assyrian king was, as we may gather from an Assyrian text, (9) an act of filial piety, and there was added a note of general sadness at which all nature mourned :

‘ The channels complain  
and the watercourses respond :  
Of trees and fruit  
the face is darkened ’.

We know however from other texts to which Frankfort has not had occasion to refer that mysterious ceremonies did take place at royal burials in Assyria. E. Dhorme (10) has commented on an Assyrian tablet which relates that a lady of the Palace whose function was apparently to serve as the dead king's bride spent some time in the tomb before it was finally closed. One of the essential parts of the ceremony was the *taklimtu* or monsterness of the body which had previously been hidden. The lady herself conducted a ritual of purification, washed and kissed the dead king's feet, passed three times round the bed and, after substituting an image in her place, was released from her duties. Parallel with that text there is a remarkable letter written in the reign of Esarrhaddon (11) describing how one Damqi and a lady of the court were sacrificed as a substitute for the king. Another text from Assur records the sacrifice of a virgin kid to Ereshkigal, queen of the shades, as substitute for a man. The animal was treated like a corpse,—embalmed, dressed, shod and adorned with eye-paint to give it the aspect of its substitute at the monsterness. These texts and a few other scattered references (12) offer about all the textual light which we may glean if we would attempt to understand the nature of the so-called Royal Burials discovered at Ur and at Kish, and presumed to have occurred at Khafajah in the early dynastic period, about two millennia before the reign of Esarrhaddon. But they do lend colour to the view that the bodies in the shaft graves at Ur were not necessarily those of kings or Royal Princes—though whether they were substitute-burials or the bodies of persons who had participated in a fertility right connected with the sacred marriage is still in dispute. Frankfort himself inclines to the theory that the death pits at Ur were substitutions for the kings. It is interesting to note that some of the regalia within these tombs, especially the golden pick-axes, were probably symbols closely connected with the god Enlil, associated in the early Sumerian literature with the Creation of mankind (13).

Within the limits of this book Frankfort has discussed a wide range of ancient beliefs ; he has marshalled his evidence skilfully and succeeded in making a connected unity out of the complex and perplexing variety of evidence which has come up for examination. Towards the end of the book he has a number of well chosen remarks to make about the ‘ suffering gods ’ of Mesopotamia ; and in an excursus on Tammuz, Adonis and Osiris he stresses the need to differentiate the peculiarly individualistic story-patterns associated with each of these divinities in Mesopotamia, Syria and Egypt, and calls attention to the need for avoiding facile assumptions about a common origin. Yet it is interesting to find that, when reviewing the part played by the wild boar as the common enemy of each of these gods, and after noticing the evidence for

<sup>9</sup> Quoted by Frankfort on p. 244.

<sup>10</sup> *Revue d'assyriologie et d'archéologie orientale*, 38, nos. 2-4.

<sup>11</sup> Quoted by Frankfort on p. 264.

<sup>12</sup> See also Sidney Smith in *J.R.A.S.* 1928, p. 849, f.

<sup>13</sup> S. N. Kramer, *Sumerian Mythology*, 51 ff. on the creation of the pick-axe.

a historical connexion between Osiris and Assur, he is forced, almost against his will, to recognize a continuous chain of thought in these three aspects of Mesopotamian, Egyptian and Syrian religion.

What exactly, we may ask, has Frankfort achieved in writing this book? I think it is this. He has succeeded in presenting to us both Egyptian and Mesopotamian religion in a coherent picture composed as an organic unity, with certain well defined and distinctive characteristics which differentiate them from any other of the important religious systems known to mankind. And he has achieved this by resolutely setting his face against considering the religion of these two countries as a gradual accretion of unconnected beliefs, whether acquired from a number of different localities or, as Kees would suppose of Egypt, gradually built up by a venal priesthood. He is not much concerned in showing the historical development: his purpose has been to show that from Early Dynastic times down to the advent of Hellenism each of these two religions had a perfectly distinct and recognisable character of its own, and that each was unified by certain basic concepts inherent in the mental and emotional make-up of the people. The same continuity which may be discerned in the arts, the economy, and the structure of society within the river valleys of the Nile, Tigris and Euphrates is found to apply to their religion too. In this respect he has managed to preserve a fine balance of mind, avoiding the temptation so freely indulged in by authorities such as Sethe and Kees to account for various aspects of religious belief by making hypothetical reconstructions of more ancient systems on the basis of the earliest known distributions of certain symbols. There has been no need to set the delta at loggerheads with upper Egypt. Frankfort has conversely avoided the temptation of considering each religion as conforming to some widespread ancient pattern of belief, and of forcing analogies from disparate material or cultures which have a unity and identity of their own. In this respect he criticizes the views of scholars such as Engnell, Hocart and Widegren. Yet he himself has at times been unable to avoid the methods of those whom he criticizes, because it is impossible to deny the interplay between one civilization and another. The transmission of religious thought has in the end had little regard for national, ethnic or geographical frontiers, and there remains some irreducible nucleus of human belief which would appear to have a common ancestry deeply rooted in the unforgotten past of prehistory.

This, in my opinion, is Frankfort's most mature work—rich in erudition, clear and balanced in presentation, with a discerning eye for certain basic concepts which give each religion its unity. His outlook is rational and free from prejudice, yet sensitive to the fine nuances of religious thought. The result is a tribute to his perceptive industry and to his capacity for absorbing polyglot learning and yet retaining his own originality of mind and individual method of approach. Not the least valuable parts of his work are the footnotes—that happy refuge of the scholar, often the repository of his best thinking, politely banished to the end of the book so as not to offend the untutored eye. Here we have a mine of references doubly valuable on account of the timely and enlightening comment which they contain.

The study of kingship in Egypt and western Asia is a profoundly moving story for all who are interested in the spiritual and mental development of man. Here we may follow the course of human striving and human suffering and perceive the mood of ancient man in his anxieties and his exaltation giving expression to the entire range of the human emotions:

*Sunt lachrimae rerum et mentem mortalia tangunt.*

The tale of ancient religion is scarred by the power of unreason, by the binding misery of fearful *tabus*, by the premeditated enforcement of human cruelty and suffering for the



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sake of preconceived notions about the way of ultimate salvation. Yet an awareness of other values shines like a beacon light through the darkness and emboldens us to hope :

Rejoice thou entire land, the goodly time has come

A lord is appointed in all countries. . . .

O all ye righteous, come and behold!

Truth has repressed falsehood.

The sinners are fallen on their faces

All that are covetous are turned back.

That was the song composed in Egypt at the accession of Merenptah. And of Hammurabi in Babylon it was said that he was called by Anu and Marduk 'to make justice appear in the land, to destroy the evil and the sinful, to prevent the strong from oppressing the weak'. Nor should we forget one New Year's day in the twenty-fourth century B.C.

That day when the king entered the temple . . .

And for seven days

The servant competed with her mistress

The domestic was on a par with his master.

In the town the mighty and the humble slept side by side ;

An evil tongue changed its words to good ones.

All evil was driven out.

He (Gudea) paid heed to the laws of Nanshe and Ningirsu,

Left not the orphan at the rich man's mercy

Left not the widow at the mercy of the strong.

The house which had no son—

Its daughter became its heir.

Days of righteousness dawned for him,

And on the neck of lawlessness and rebels he set his foot.

Glory to God in the highest, and on earth peace, good will toward men. The King is dead, Long live the King !

# Men, Machines and History\*

by O. G. S. CRAWFORD

DURING the present century there has appeared a new kind of history, concerned not with parts but with the whole of human history. There were, of course, before this, many attempts to write 'Universal History'; but they consisted merely in lumping together in a single continuous narrative condensed histories of the chief countries of the Old World. This synthetic or accretive method has the same defects when concerned with the sequence of time as it has when concerned with space. You cannot make a correct map of the world or of any part of it simply by sticking together on a single sheet the plans of separate portions surveyed independently by different people, with no comprehensive framework binding together the whole area. Nor, to vary the metaphor, is a cathedral constructed by putting several parish churches together beneath one roof. The new history is related to the Old Universal Histories exactly as is a modern map of the world to such medieval concoctions as the Hereford map.

The analogy, is, as always, imperfect. Most of the early cartographers realized the need of some rigid terrestrial framework and some, like Ptolemy, constructed one which was correct in principle and defective only from errors of observation and inference, or lack of data. The Arab geographers constructed an artificial system from Ptolemy and their own imagination. Modern maps are correct because they are based solely on the objective facts of observation controlled by mathematics.

To what extent is it possible to construct an objective framework for human history? Before attempting to answer this question we must be quite clear about what we are dealing with. The unit of cartography is the surface of the earth; of the new history, man as a species and societies of men. That we are able to stand back and see this vista at all is entirely due to the discoveries of prehistoric archaeology during the last hundred years. Those discoveries put man in his place in nature, replacing the speculations hitherto dominant by objective facts consisting chiefly of tools (such as stone axes) and skeletal remains.

Archaeology, by concerning itself exclusively with the remains of material culture, has thus created an objective framework for the history of man. It shows how a primitive anthropoid climbed up from the apes by not only using and making tools (which apes can do) but also by thinking about them and improving them (1). The range of

\* (1) *Men, machines and history*, by S. Lilley. Cobbett Press (Past and Present Series: double volume). 240 pages. Price 10s 6d.

(2) Ikhnaton: the Great Man versus the Culture Process, by Leslie A. White [University of Michigan]. *Journal of the American Oriental Society*, Vol. 68, No. 2, April-June 1948, 91-114.

(3) On the use of tools by Primates, by Leslie A. White, *Journal of Comparative Psychology*, Vol. 34, No. 3, December, 1942, 369-74.

(4) *Joseph Stalin: an interview with the German author, Emil Ludwig*; printed by the Cooperative Publishing Society of Foreign Workers in the U.S.S.R., Moscow, 1932.

<sup>1</sup> For the development of this thesis see Leslie White, 'On the use of tools by primates', *Journal of Comparative Psychology*, Vol. 34, No. 3, December 1942, pp. 369-74; and his note, condensing his remarks, in *ANTIQUITY*, XXII, pp. 210-11.



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consciousness is thus gradually extended from a moment to millennia. Our modern consciousness of history is but an extension of our ancestral anthropoid's thoughts about his flint tools and how to improve them and pass on his knowledge. Language of course was a fundamental factor in this advance. Next we see man passing on from the hunter to the agriculturist and thus by rapid stages to urban civilization.

I wish to stress the objective character of the framework which archaeology has thus created. It is, of course, open to critics of a certain school to say that this framework is merely an inference from the facts. The criticism comes badly from those whose framework is often, on the most generous assumption, constructed of inferences unsupported by any objective facts, and it may be ignored.

From all this it follows that the new history is essentially of the kind called technologic. It is concerned with the fundamental needs of life—food, clothing and shelter, transport, raw materials, trade. It recognizes the class divisions which have bedevilled history for some five thousand years, and measures the level of civilization 'not only by its peaks of intellectual culture, but also by the standard of living of its whole people (2)'. Thus it redresses the balance of the other histories which have for the most part been concerned with the culture of a single dominant class. That is not unnatural. Down to modern times historians have been dependent on histories and annals which were themselves compiled by members of a ruling class which 'saw the world only from the point of view of consumers'. They do not therefore tell us the sort of things that the new historians want to know. There are pages and pages of politics and war, but not a word about how their daily bread was ground (3). It has been said that huge tracts of history are biased because 'God has written all the books'; it is equally true that all ancient history is equally biased because the books have been written by members of the ruling classes, interested 'in the art of exploitation, of government, of extracting the last available grain of corn from the peasant', but 'incapable of advancing the technical methods of society (4)', and therefore not interested in describing them—and indeed too ignorant of the processes to do so (5).

Modern discoveries and the publication of records has done much to redress the balance, particularly in economic matters. We catch a glimpse of the ordinary citizen at work or (more often) at loggerheads with another such. From the exhaustive study of all available records of a medieval town we can build up in our minds a fairly accurate picture of its social life. But here archaeology can often greatly help. What, for instance can we gather from the whole mass of documentary evidence between say A.D. 600 and 1066 that gives us the slightest idea of what a Saxon village or town was like? Practically nothing. Yet a couple of seasons' excavation on the site of Saxon Southampton tells us a great deal. It shows that this community was, so far as material culture is concerned, on or even below the level of similar British communities during the prehistoric Iron Age, a thousand years earlier. (Their huts were no better and their pottery definitely inferior).

The new history is concerned with the *whole* of any given society and not merely with the dominant class. It judges any such society by the cold criteria of their material

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<sup>2</sup> Lilley, pp. 35-6.

<sup>3</sup> The history of water-mills is an example. It had to be reconstructed from a few meagre references in the ancient writers who were no more interested in such matters than are their modern students.

<sup>4</sup> Lilley, p. 18.

<sup>5</sup> Malthus (1766-1834) said the same thing.

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culture, which includes of course works of art (if any). It is not impressed by religious labels, but only by behaviour. For instance, the unbiased historian sees much in common between Ireland and Abyssinia during the medieval period. Both societies were called Christian; both were geographically remote from civilized regions; both were literate but barbaric. Neither society made any effective contribution to technical progress. Both however are extremely interesting to the new historian because their fairly full documentation gives a vivid picture of the *sort of* life led by similar societies in undocumented periods.

It is not possible to form a true picture of these and other societies if one ignores their material culture, but only by comparing them with each other on that basis. The older historians and travellers, blinded by prejudice or obliged to conform to it for fear of consequences, regarded Christian societies everywhere as *ipso facto* superior to others. Muhammadans did the same. To each the societies of the other belief were infidel. Yet the discerning historian sees little if any moral or cultural differences that can be attributed to religion. The Christian Abyssinians castrated their enemies and gloated over it; and the records were set down by priests. The Muhammadan Turks impaled them alive down to the 19th century. The lives of the common people, made miserable by the misdeeds of their rulers, were much the same everywhere.

To sum up the argument so far :—The new history differs in kind from the older syncretic Universal Histories. It is an outcome of the scientific study of archaeology which provides an objective framework for the history of man, based upon the remains of his material culture. The picture thus composed is more complete than one drawn from documentary sources only, for these emanate from members of one section, the ruling class, and reflect its point of view. The new method lays bare the foundations upon which the superstructure rests and without which it cannot exist.

The argument now advances a stage. To continue the analogy one would say that (in the development of human society) the plan of the foundations determines the character of the superstructure. If the plan is rectangular, so are the upper stories. Translated into the language of history, this may be expressed by saying that technologic factors determine the social and political organization. That this is so may be seen most clearly in the early stages of human history. The primitive societies of nomads and hunters are based upon kinship ties. The wandering life prohibits complex political organization. But as soon as the basis of subsistence changes to agriculture, a new form of society immediately begins to develop. 'The transition from primitive to civil (6) society was brought about by technological advance, specifically, by the development of agriculture, supplemented—though not everywhere—by the domestication of animals. The maturation of the agricultural arts produced the following chain of sequences: increased food supply, increase in population, increase in population density and in size of political groupings, diversion of human labour from food-producing to specialized arts and crafts, a new type of exchange and distribution of goods, money and markets, economic classes, and so on'. The civil society which agriculture thus brought into existence 'required a special mechanism to coordinate the various segments and classes of society, and to integrate them into a coherent and effective whole. Such an integrated mechanism was produced. It was the "State Church", i.e., a mechanism having temporal and ecclesiastical aspects'.

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<sup>6</sup> 'The evolution of society is marked by two great stages: primitive or tribal, and civil or national. The tribe and clan are characteristics of primitive society (although the clan is by no means universal); the political state characterizes *civil* society'. White, *The Great Man*, p. 95, where the passage quoted above also occurs.



We need not for the moment follow Professor White's particular argument any further. The first part (summarized above) will already be familiar to readers of Professor Gordon Childe's books, and probably therefore to most readers of *ANTIQUITY*; and the rest leads up to the discussion which is the special subject of the author's article, namely, the rôle of the Great Man in history. What we wish to make clear, even at the risk of wearisome repetition, is the fact, securely established by archaeology, that the great transition from primitive to civil society came about as a result of the development of agriculture. The (economic) foundations determined the (political) superstructure. If that happened once, it should happen again. Any comparable change in the mode of production should profoundly change also the political organization of society.

The change from primitive to civil society was the outcome of what Professor Childe (who first described it) called the First, or Neolithic, Revolution. Mr Lilley calls it the First Industrial Revolution; and his Book, which gives a condensed summary of it, is more particularly concerned with the Second Industrial Revolution. It is therefore the sequel to Professor Childe's studies—a sequel whose need was obvious, in order to carry the story down to modern times. It is therefore the last of a series of books which, taken as a whole, present a fairly complete outline of what I have here called the new history. The Second Industrial Revolution is divided into four phases: Embryo (A.D. 500–1440): Childhood (A.D. 1440–1660): Youth (A.D. 1660–1815): Maturity (A.D. 1815–1918). The factor which distinguishes the Second from the First is the application of mechanical, instead of human or animal, power to production, beginning with water and wind, and advancing to steam, electricity, oil (and now presumably nuclear fission). Naturally the book deals chiefly with mechanical inventions; but, unlike other such books, it is not merely a dry catalogue of dates and facts, for the social implications and effects are constantly pointed out. The author's treatment of printing is an excellent example. Books, 'which had hitherto been valuable treasures because of the costly process of hand copying, [were brought] within the range of wider classes of people. Within a century or two the ambitious craftsman had the means to study in books the accumulated study of others and to apply it to his own problems. This was a revolution comparable to that brought about by the introduction of iron. Iron democratized physical tools; printing did the same for the tools of thought. This was an important factor in the increasing rate of invention that came in the following centuries (7). The claim is substantiated by many of the illustrations and references in the book.

It should be noted that Mr Lilley's treatment of history is essentially that of the archaeologist (8). He deals with the remains of material culture and from them builds a historical narrative. His is the sort of book that, deprived of documents, an archaeologist of the future might write, if the evidence were available to him. It is one of a series called 'Past and Present', whose editorial board consists of Professors Gordon Childe and Benjamin B. Farrington and Mr Sidney Herbert. Others in the series are Dr Grahame Clark's *From Savagery to Civilisation* (reviewed in *ANTIQUITY*, XXII, 1948, 217–8), Dr Cecil Curwen's, *Plough and Pasture*, and Professor Gordon Childe's *History*. It is aimed to publish forty books in all. If the others are all as good as these, the series will take its place in the history of publishing with 'Everyman's' and the Home University Library of our youth.

At this point we might well conclude this section. Some readers will certainly

<sup>7</sup> Lilley, p. 47.

<sup>8</sup> A minor point of criticism is concerned with his references to the wheeled plough, which need revision.

think that it would have been better to do so. They are those who do not approve of the attempt to interpret contemporary or recent events in the light of the past. Admittedly there are dangers in mixing past, present and future, and there are occasions when it is desirable, or more politic, to remain aloof in the Ivory Tower. But this is hardly one of them. A review is nothing if not the expression of a personal opinion, and an opportunity for airing views, and so testing their validity.

Does it not follow almost automatically from the historical facts above enunciated that the Second Industrial Revolution should change the political organization of human society? If the First did so, surely it *must*? Like it, the Second has already enormously increased the population, and raised the standard of living. Will it not ultimately transform society as completely as did the invention of agriculture? What will life be like if it does? To these questions there are of course no answers except guesses. One thing is fairly certain and (to the present writer at any rate) reassuring—the process of transformation, though it may be much more rapid than the other earlier one, will be spread out over a considerable period, and will not take place everywhere at once. Even today, some seven thousand years afterwards, there still survive quite a few non-agricultural societies (Esquimaux, Bedouin, Beja, pygmies); and it seems probable that in remote places there may long survive a few non-collective farmers living in unprefabricated houses and eating untinned food. If amongst them there is room for uncontrolled archaeologists, it is probable that not only the present writer but many also of his readers will be found there.

These, however, are merely the irrelevant personal reactions of one whose life began and whose habits were formed in a world that is passing away. If we can judge by the reactions of the nomads of Arabia to-day, the paleolithic hunters probably regarded the way of life of the first farmers with similar aversion.

Human history thus falls naturally into three periods:—A very long period of hunting and collecting, a short period of some ten millennia of food-production based on agriculture and of industry based on human and animal power, and a period, which is just beginning, of both agriculture and industry based upon mechanical power. It is archaeology which has discovered the whole of the first period and the greater part of the second, and so created the new history.

The difference between the two kinds of history—the old and the new—is essentially one of scale—in time. Except during the two great revolutions mentioned there is not, in the short periods covered by national histories, time enough to show much technical advance. The scale is too large. Again there is a close analogy to be drawn with spatial scale represented by maps; a map of Switzerland covers too small an area on too large a scale to show the river system of Europe or the structural connexion of the Alps with the Pyrenees and the Carpathians. It is not of course intended to do so; but it does not therefore follow that a small scale map of Europe bringing out these connexions is not required, or that it is not a map at all. Yet there are some who would deny the name of history altogether to that small scale, long range kind whose unit is mankind, not parts of it. Of course both are needed; the small scale history of man is as necessary as the large scale histories of nations, just as there are times when a map of Switzerland is more useful than a map of Europe. But we shall probably get more out of our map of Switzerland if we have also studied a map of Europe; the Rhine, for instance, will have more meaning. The analogy is closer if for topographical maps are substituted geological ones, which reveal structure and suggest (to the trained eye) causation.

The analogy chosen has force because it is drawn from the other dimension, space; history is concerned with time. Both are quantitative concepts and susceptible of

quantitative treatment. Scales are merely mathematical proportions, whether of time or space.

Professor White, as we have seen, accepts the view that technological advance is the mainspring of human history. His essay is designed to show that 'cultural advance' is 'not the work of a relatively few gifted individuals', and that the Great Men of the history books only seem to shape civilization. He traces the Great Man idea back to the magician who claims to be able to influence nature, and to his successors whose claims were greater, and were accepted. Some were regarded as better *shamams* than others; they had more 'power'. 'On higher cultural levels we find chiefs and priests; then kings and emperors, popes and potentates . . . As culture advances, the exceptional person increases in stature; great cultures [it is thought] can be built only by Great Men. Like Yahweh who made the light merely by calling for it, the Great Men make society and history by exercising their inherent genius'. This blind exaggeration of the power of the individual persists even today; and Professor White has selected for the purpose of demonstrating its falseness the example of Ikhnaton, 'the Heretic King'. He examines the claims made and shows—we think conclusively—that of Ikhnaton himself we know little or nothing, and that his character has been built up entirely by inference from the known events of his reign, often wrongly interpreted. The clinical diagnosis of his body is inconclusive, and it is even uncertain whether the body examined was that of Ikhnaton or not. 'The stirring events of [his] reign can be accounted for as a part of a great process of cultural change and development. And we can explain this process in terms of itself . . . in exactly the same way that we can account for the changes brought about in American culture by the introduction of the automobile. We do not need to call upon great men or upon psychological forces to make them intelligible'.

Ikhnaton, in fact, was merely the figurehead in an age-long struggle for power between State and Chrch—the two integrative and rival mechanisms of society. The State party was alarmed at the growing temporal power of the priests and decided to have a show-down. For a time they succeeded, but in the end the priests won. Their power was too firmly established upon this earth to be overthrown, and eventually the throne was usurped by the high priest of Amon who became successively viceroy of Nubia, commander-in-chief of the army, vizier of Upper Egypt and Pharaoh.

The anthropomorphic view interprets the known facts of history—of the culture-process (9), as Professor White calls it—by the pseudofacts of psychology, the known by the unknown. 'A worse error of reasoning would be hard to find—within or outside the field of scholarship'. What, he asks, 'could a man . . . of exceptional quality and ability have done in this or *any* situation except respond to it—to work with the materials at hand, to try to cope with the problems confronting him, in short, to fit himself to the culture process that is his context? A man of superior . . . make-up might have made a better, i.e., more effective, response than one of inferior brains and physique, but the pattern of his response would have been substantially the same because it would have been determined by the same cultural situation . . . So that even if Ikhnaton were an organism of exceptionally fine quality, this fact would not at all suffice to explain the events of his reign'. Actually, of course, as has been said, we know practically nothing about the quality of Ikhnaton, and what has been offered to us by the Egyptologists is either unjustified inference or pure fiction.

It is very interesting to compare this view of the rôle of the Great Man with that expressed by a Great Man himself. Opinions differ of course very widely about the

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<sup>9</sup> Prof. White uses the word 'culture-process' to refer to culture in its dynamic rather than its static aspect.



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character of Marshal Stalin, but very few, we think, would be so bold or so foolish as to deny him a place amongst the great men of history. On December 13th, 1931, the German journalist Emil Ludwig had an interview with Stalin and asked him several questions of a historical kind. The subject was introduced by Ludwig, who, after disclaiming any knowledge about economics but claiming to be a good judge of people, asked Stalin whether he thought there was any parallel between him and Peter the Great? Did Stalin regard himself as continuing the cause of Peter the Great? Stalin did not; historical parallels, he said, are always dangerous, and this question was absurd. He proceeded to show why. Lenin was mentioned, and this gave Ludwig the opportunity of suggesting that the historical importance of Lenin (admitted by Stalin) contradicted the Marxist view which (according to Ludwig) 'denied that personalities play an important rôle in history'. The reply must be quoted in full.

'Marxism', said Stalin, 'does not deny that prominent personalities play an important rôle, nor the fact that history is made by people. In *The Poverty of Philosophy* and in other works of Marx you will find it stated that it is people who make history. But of course people do not make history according to their fancy or the promptings of their imagination. Every new generation encounters definite conditions already existing, ready made when that generation was born. And if great people are worth anything at all, it is only to the extent that they correctly understand these conditions and know how to alter them. If they fail to understand these conditions and try to change them according to their own fancies, they will put themselves in a quixotic position. So you will see that, precisely according to Marx, people must *not* be contrasted to conditions. It is people who make history, but they make it only to the extent that they correctly understand the conditions they found ready-made, and to the extent that they know how to change those conditions. That at least is the way we Russian Bolsheviks understand Marx; and we have been studying Marx for a good many years'.

How intriguing is the contrast here brought out! Both Marshal Stalin and Professor White break up a given historical situation into the same component parts (so it seems to me)—on one hand the 'definite conditions already existing' (Stalin) and the 'cultural situation' (White) which the man on the other hand must understand and which are his context. But how different the opinions about the reaction of the man to the situation! White is a cultural determinist; the cultural situation determines the response of the man, and the pattern of this response will be substantially the same whether the man be superior or inferior. Stalin is the champion of the Great Man who can, if and in so far as he correctly assesses the situation, alter it. Perhaps the contrast is less profound than at first appears. Is not the existence of classes with opposed interests common ground to both? And is not the only difference, concerned with the extent to which an individual can influence the class struggle? After all it is not so much Ikhnoton (who was obviously a puppet) as the historians who have overrated his importance that Professor White is out after. He is not out to decry the Great Man so much as to confute those who do not understand correctly his historical rôle. Some (but not the present writer) will be startled to hear a voice from capitalist America decrying the historical rôle of the individual, and one from the U.S.S.R. proclaiming that 'men are at some times masters of their fate'. But did not Gordon Childe once write a book called 'Man Makes Himself'? Perhaps those startled ones, whose study of Marxism is probably derived from the letters written to *The Times* by infuriated and frustrated opponents, may still have something to learn about it!

# Important New Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review*

- THE MEARE LAKE VILLAGE : a full description of the excavations and the relics from the eastern half of the west village, 1910-1933, by ARTHUR BULLEID and HAROLD ST. GEORGE GRAY. Vol. I, 1948. 42s. (To be obtained from H. St. George Gray, Taunton Castle, Somerset).
- ROMAN WAYS IN THE WEALD : illustrated with maps, diagrams and photographs, by I. D. MARGARY. Phoenix House, London, 1948. 25s. [An account of field-work, excavation and air-photograph, extending over many years : foreword by the Editor of *ANTIQUITY*].
- ALEXANDER THE GREAT, by W. W. TARN. Cambridge University Press, 2 vols, 1948 (Vol. I, 10s 6d ; Vol. II, 30s).
- PERSEPOLIS TREASURY-TABLETS, by GEORGE G. CAMERON. Univ. of Chicago Press [Oriental Institute Publications, Vol. LXV] : London ; Cambridge University Press. \$12.00.
- ONCE AGAIN ARTHUR'S BATTLES, by KENNETH JACKSON. *Modern Philology*, Vol. XLIII, No. 1, August 1945, 44-57.
- STRATIGRAPHIE COMPAREE ET CHRONOLOGIE DE L'ASIE OCCIDENTALE (3<sup>e</sup> et 2<sup>e</sup> millénaires), par CLAUDE F. A. SCHAEFFER. Published on behalf of the Griffith Institute, Ashmolean Museum, Oxford, by Geoffrey Cumberlege, Oxford University Press, London, 1948. 84s. [A masterly synthesis of all important excavated sites in this area, including his own at Ras Shamra].
- EXCAVATIONS AT THE JEWRY WALL SITE, LEICESTER, by KATHLEEN M. KENYON. Printed at the Oxford University Press for the Society of Antiquaries of London and the Corporation of the city of Leicester : sold by Quaritch, 11 Grafton St., W.1. 30s.
- ANCIENT INDIA, Number 4 (double number of 321 pages) ; to be obtained from the Director-General of Archaeology in India, New Delhi, India. Subscription rates for 4 numbers (2 years), 7 rupees or 11 shillings. [And remarkably good value ! Articles by Gordon Childe (megaliths), Stuart Piggott (pins and a mace-head from Harappā), Ghosh (Taxila, with plan), Wheeler and Piggott (Iran and India in pre-Islamic Times) and by Wheeler on Mysore megaliths].
- SUMER, Vol. v, No. 1, January 1948. Published by the Directorate-General of Antiquities, Baghdad, Iraq.
- A HISTORY OF CAST IRON IN ARCHITECTURE, by JOHN GLOAG and DEREK BRIDGWATER. George Allen and Unwin, 1948. 63s. [A very fully illustrated account of the subject, covering much neglected Victorian iron-work, railings, etc.].
- HISTORY OF ANCIENT GEOGRAPHY, by J. OLIVER THOMSON. Cambridge University Press. 42s.
- GALLO-BRITISH COLONIES : THE AISLED ROUND-HOUSE CULTURE IN THE NORTH, by SIR LINDSAY SCOTT. *Proc. Prehistoric Society*, NS. Vol. XIV, 1948, 46-125. [A very important study of prehistoric house-plans by the President of the Prehistoric Society, who has excavated many of those described himself. This will become a *locus classicus*].

## Reviews

TOPOGRAPHY OF ROMAN SCOTLAND, NORTH OF THE ANTONINE WALL. By O. G. S. CRAWFORD. 4to, pp. I-XII, I-162, figs. 1-32, pls. I-XXI, one folder and two maps on end-papers. Cambridge University Press, 1949. Price, 25 shillings.

This admirable and beautifully reproduced survey of the Roman roads, forts and marching-camps north of the Antonine Wall is the first comprehensive study of this rich and remarkable material since General Roy's great folio. Its author is one whose single-handed contributions to the antiquarian topography of Britain have placed him in a unique position among archaeologists and have set a standard of clarity and vigour peculiarly his own.

The study records the state of knowledge in 1943. Since then a good deal has been discovered in these very regions, more marching-camps at Ardoch, new marching-camps at Bridge of Allan and in the Mearns; and the fort-site at Bochart, near Callender, has been verified by excavation. But the value of Mr Crawford's work, as so often, is none the less still the challenge which it presents. The new discoveries at Bridge of Allan may seem to focus the view of the Forth crossing: in particular, they ask whether the Romans did choose the seriously overlooked site below the shadow of Stirling Castle, or a clearer site north of the river. In other words, was the bridge-head north or south of the Forth? Again, in clearing out of the way, most satisfactorily, existing identifications of Roman roads in the neighbourhood of Dealginross and Fendoch, the question is raised whether there were any at all and if so, where? The account of the signal-stations from Ardoch to Gask (why is there, however, no map of the Gask area?) raises the question whether the differences not merely in size but in plan are differences in age: just as the very interesting study of the Bertha region raises questions of the same kind. Roy, by the way, calls the marching-camp Grassy Walls, which seems more reasonable as a place-name than Grassy Wells, the author's version *passim*, and one would like to have seen the reason for the change discussed. Another site worth a text-figure is Cardean, of which the account is difficult to follow unless one knows the ground already. If Fochabers was worth a text-figure, so was it.

The historical discussion is weakened by the fact that it takes little account of other than Agricola's campaigns. If one may take liberties with Horace, *vixere fortes post Agamemnona!* And the relationship of the Ardoch marching-camp to the signal-station will suggest that we might make a start in definition by testing there. But I do not think that Raedykes has before been selected for the battle of Mons Graupius by a recognized authority: and there is much in the suggestion to commend it. I. A. RICHMOND.

THE ALPHABET: A Key to the History of Mankind. By DAVID DIRINGER, D.LITT. *Hutchinson's Scientific and Technical Publications*. London, 1948. pp. XII, 607. Figs. 256. 50s net.

This is not so much a book as an encyclopaedia. Here the last word has been said for many years to come on the Alphabet and the history of writing. It is sixty years since Isaac Taylor's book on the Alphabet appeared, and during the last half-century great advances have been made in this field. Every available scrap of new knowledge has been carefully gathered, sifted, and scientifically arranged by Dr Diringer in the



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volume of over 600 pages which now takes its rightful place as the definitive authority on everything concerning man's most important invention. Dr Diringen was already well-known to students of Semitic epigraphy for his Italian publications on North-Semitic inscriptions, and for his book on the Alphabet, also in Italian. Now he has produced, in English, an entirely new book on the Alphabet and on the history of writing in general. Great praise is due to the liberality and enterprise of the publishers, Messrs Hutchinson, for the lavish scale on which the book is illustrated, adding enormously to its value.

The book is divided into two parts. The first, occupying 191 pages, deals with what Dr Diringen calls 'Non-alphabetic Systems of Writing'; under this head are included the various forms of cuneiform writing, the Egyptian hieroglyphic, hieratic, and demotic scripts, the Cretan scripts, the Indus Valley script, Hittite, Chinese, Central American, and other ideographic scripts. A separate chapter is given to syllabic systems of writing, and the last chapter of this section deals with what Dr Diringen calls quasi-alphabetic scripts, including early Persian cuneiform, and Meroitic scripts. The order of treatment in this section is mainly chronological. Some slight, but unavoidable cross-classification is occasioned by the separation of syllabic scripts from the earlier 'mixed' systems which, although pictographic in origin, became largely syllabic in the course of their development.

The second section of the book, from p. 195 to the end, deals with the origin and growth of the alphabetic principle of writing, and discusses in turn all the known forms of alphabet which have been or are still employed in writing. The Indian branch of alphabetic forms is treated with a fulness not hitherto found in any book on the subject. The two chapters devoted to the immense variety of Indian alphabets contain a mine of information of the greatest interest, here brought together for the first time.

That part of the book which will probably be most closely examined is the chapter which deals with the still undecided question of the origin of the alphabet. The burning point of interest at the present moment is to be found in the discussion raging round the Byblos inscriptions, concerning which a wide difference of opinion exists between MM. Dunand and Dhorme. M. Dunand believes that the so-called 'pseudo-hieroglyphic' script, in which a group of inscriptions discovered by him at Byblos in 1929 and succeeding years, were written, is the 'incunable', or cradle, of the alphabet. He considers that these inscriptions belong to the period of the 12th or 13th dynasties, and that they were contemporary with the existence of the early Phoenician alphabet. Hence Dunand is prepared to date the invention of the alphabet as far back as the 19th century B.C., five or six centuries earlier than any date so far proposed for that event. Professor Dhorme, however, who has deciphered and translated the Byblos inscriptions, though his results have so far not been published\*, does not agree with Dunand on this very early dating of the invention of the alphabet. Dr Diringen sums up his own position on this question as follows: 'According to my opinion, we can date the origin of the North Semitic alphabet, or of its prototype, which we can call proto-Semitic alphabet, in the 2nd quarter of the 2nd millennium B.C. In other words, the great event occurred probably in the Hyksos period, which is now commonly dated 1730-1580 B.C. There is no doubt that the political situation of the Near East in that period favoured the creation of a "revolutionary" writing, a script which we can properly term "democratic" in distinction from the "theocratic" scripts of Egypt, Mesopotamia, or China. All the other more important attempts at alphabetic writing, the early Sinaitic script, the early Byblian and the early Canaanite scripts, can also be attributed to the Hyksos period'.

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\* Since this was written Professor Dhorme has published his results in *Syria*, xxv, 1-2.



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With regard to the place of origin, Dr Diringer rejects Sinai as the home of the first attempts at alphabetic writing, and prefers Palestine or Syria. Since the impetus to this great adventure was probably utilitarian, it seems more probable that the need felt for a more expeditious means of intercourse than the cumbrous hieroglyphic or cuneiform systems should have arisen in the busy trading centres of the Palestinian or Syrian sea-board. Since the publication of Dr Diringer's book, an important article in the *Bulletin of the American Schools of Oriental Research*, no. 110, by Professor Albright has appeared, dealing with the Sinaitic inscriptions and giving translations of them. Professor Albright's conclusions are, in his own words: 'I consider the Proto-Sinaitic script as normal alphabetic Canaanite from the early 15th century B.C.'. Professor Albright has also carried considerably further the identification of the Sinaitic signs with corresponding signs in the early North-Semitic alphabets.

One valuable feature of this book is the full bibliographies which are appended to each chapter. Very little has escaped Dr Diringer's notice. In a book of this size it was inevitable that a few errors and mistakes should have remained uncorrected, but they are surprisingly few. 'Meisler' appears for 'Maisler', and on p. 564 the source of information concerning the strange Oberi Okaiame script is erroneously given as the *African Journal*, instead of *Africa*, the well-known quarterly of the International Institute of African Languages and Cultures. The uninitiated reader, and many such will be using this book, might find the meaning of the word 'syllabary' a little obscure, e.g., on p. 36 and elsewhere. Also the term 'stimulus-diffusion' might profitably have been further elucidated. The Index contains no references to authors, but their inclusion might have swollen it to undue proportions. But these are trifles in comparison with the magnitude of the achievement which this book represents. It will long remain the standard authority on everything relating to the history of writing. S. H. HOOKE.

## ESSAI SUR LES ORIGINES DE LA MINIATURE DITE IRLANDAISE.

By F. MASAI. [des publications de Scriptorium, vol. 1]. pp. 146, plates. Bruxelles: Editions 'Erasmus', 226 Avenue des Alliés; 1947. Price 450 francs.

This study of the origin and sources of early Insular miniatures is best summarized in the author's own conclusion that henceforth it will be more precise to speak, not of Irish, but of Anglo-Irish miniature painting. 'Although the Scotti', he continues, 'practised this art during several centuries, thereby establishing a certain right to retain the older designation, the English, who were creators of the style and produced its greatest masterpieces, have no less vindicated the claim to give it their name' (p. 136). This conclusion is less novel than a superficial reading of M. Masai's work would suggest, but his thesis, stated polemically and developed with the aid of a learned array of authorities, is a useful summary of the trend of modern opinion, setting out a case which can no longer be ignored or relegated to footnotes. It must, however, be noted that the citations are eclectic and that the author does not hesitate to suppress evidence unfavourable to his cause. One instance must suffice. Dismissing the traditional ascriptions of the Book of Dimma and the Book of Moling, Masai (p. 41) writes: 'The name of Dimma is too common and the whole MS. speaks against a great antiquity. Zimmermann boldly attributes it to the second half of the 8th century. An interesting observation of Lowe should also be noted: the whole superscription on fol. 74<sup>v</sup> is, according to the eminent palaeographer, a late addition. Furthermore in three other places (Lowe is again cited as the authority) the name of Dimma is written over an erasure . . . The other codex . . . bears at the end of St John's Gospel the following colophon *nomen autem*



*scriptoris mulling dicitur.* This Mulling has been identified with the Bishop of Ferns, who died in 696. The decoration does not support this judgment and, as Zimmermann observes, the text has every appearance of a copy and must be of this second half of the 8th century at the earliest'. The ignoring of Lowe's careful judgment (*Codices Latini antiquiores*, II, no. 276), that 'the identification with St. Moling (†696) the founder of the monastery, seems palaeographically possible', is difficult to explain, even though the author may have good reasons to dissent from it.

*The Book of Durrow*, the oldest and one of the finest of the illustrated Insular MSS. is fully discussed and the case for its Northumbrian origin is fairly stated. E. A. Lowe (*Codices Latini antiquiores* II, p. xiv) drew attention to the orderliness of the script, a Saxon, not an Irish, feature and its palaeographical connexion with the Codex Epternacensis. F. C. Burkitt (*Journal of Theological Studies*, xxxiii, 254; *ANTIQUITY*, ix, 33) pointed out the pure Northumbrian character of the text of the Gospels, which is closely related to that of the Book of Lindisfarne and of the Codex Amiatinus. Clapham (*ANTIQUITY* VIII, 43) shewed that the sources of the ornament were Saxon and that the greater number of the motives were unknown to Irish art of the 7th century. Zimmermann (*Vor-Karolingische Miniaturen*, 124) though accepting the traditional ascription to Ireland had already noted that the Durrow style gave rise to no imitations in Ireland but found its copyists in England and the English inspired monasteries on the Continent. The last two writers, the earliest in date of publication, accepted the traditional ascription of the MS. to Ireland but this rests in the last resort on no firmer basis than the famous colophon, with its reference to Columba the writer, and Abbott (*On the Colophon of the Book of Durrow*, p. 199) had long ago pointed out that the vital words were written over an erasure. The growing weight of evidence led Lowe to the clear attribution: 'written in Northumbria by a hand trained in the Irish manner' (*Codices Latini antiquiores*, II, no. 273) and this conclusion has not been seriously impugned, though more recently T. D. Kendrick, in a book apparently not known to Masai (*Anglo-Saxon Art*, published in 1938), has cited the palaeographical evidence and, after considering Clapham's suggestion that the decoration was Northumbrian, rejected this view and restated the 'Irish' case. The archaeological material is discussed by Masai in a separate chapter and here again the evidence points in the same direction.

In the later period interest will centre on the author's treatment of the Book of Kells. His analysis is less full and his conclusion less definite (p. 124): 'all the lines of evidence converge, all point to a Northumbrian workshop, or at least to a scriptorium dependent on (dans la mouvance de) Jarrow and Lindisfarne; such as in the wider sense (à la rigueur) the abbey of Iona, might be considered'. It is difficult to agree. Leaving aside the ornament on which opinions are likely to remain more subjective, one may turn to the script and cite the opinion of Lowe (*Codices Latini antiquiores*, II, p. xii): 'The Irish scribe often behaves as if the written line were something elastic, not a fixed and determined space, which has to be filled in a particular way. He seems often guided by whim and fancy. The English scribe by comparison, is balanced and disciplined. One need only place side by side the Book of Kells and the Lindisfarne Gospels, or the Book of Mulling and the Moore Bede to see the force of these observations'. In the reviewer's opinion the arguments adduced by Masai cannot outweigh this judgment. English influence is clear both in this text and the ornament. But English customs were reaching Iona as early as the time of Adamnan. The Book of Kells may well have been written at Iona and have absorbed the various influences at work in that centre; but if this were so, it seems idle to quibble at the use of the adjective Irish to describe a product of the monastery, which was still the seat of the successor of St. Columba.



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It is impossible to consider in detail the many issues raised by this book. One omission in the bibliography has already been cited. Another is Clapham's *English Romanesque Architecture before the Conquest* which contains a more modern and authoritative treatment of the great Saxon crosses than the work of Collingwood cited on p. 64. A number of errors have crept in. Ricemarch, the son of Sulgen, was a Welsh not an Irish scribe (p. 48); like his father he was Bishop at St David's and his death is recorded under the year 1099. Alfred, who redeemed the Codex Aureus and gave it to Canterbury, was not the King but an *ealdorman* as the translation on p. 62 shews. Attention may also be drawn to the author's theory of Insular majuscule and minuscule scripts, attributing the one to England and the other to Ireland (p. 139). A full consideration of this far-reaching suggestion would require an analysis of all the MSS., not only of those with illustrations, and would, in particular, necessitate an explanation of the position of the Cathach of St. Columba, which is generally classed as majuscule and which Lowe (*Codices Latini antiquiores*, II, no. 266) is prepared to accept as being of the time of the Saint. It is, at any rate, far earlier than Christian Saxon influence in Ireland. (Cf. Proceedings of the Royal Irish Academy, XXXIII, 291). But a discussion of these points would reach far beyond the limits of this review, in which it is unnecessary to consider in detail the consequences of Masai's main conclusion, consequences which he only claims to have sketched in outline. C.A.R.R.

**HISTORY IN LEICESTER.** By COLIN D. B. ELLIS. *City of Leicester Publicity Department*, 1948. 138 pages, 25 maps and illustrations. Price 9s 6d.

Leicester and Leicestershire have been fortunate in their historians. They have had a Throsby, a Nichols and a Miss Bateson, while more recently C. J. Billson, R. W. Greaves, W. G. Hoskins, A. T. Patterson, S. H. Skillington and others have all done work of which any area might be proud.

Mr Ellis's book is something different. It is not a work of original research; it is not even a learned work (the earlier sections betray inadequate and insecure foundations), and the balance of interests, local, national and dramatic, is not always well maintained. But to criticize it as a work of scholarship or even as a history of Leicester is to misunderstand the author's aim. One feels that he loves his Leicester and is determined to make others love it too. He attempts the difficult task of weaving local and national history into a popular story that can be read and enjoyed by all. And, assisted by an easy racy style, he has succeeded. His sacrifices of completeness, of balance, and sometimes of accuracy, do not prevent the achievement of his primary aim which is, it seems, to attract. He has written an extremely attractive book and, as was no doubt his intention, it will drive many to deeper study.

Outstanding features of the book are its superb illustrations and its uniformly excellent production. And the title page bears the impress of the City of Leicester Publicity Department. Leicester is fortunate: it possesses a University department of Local History and an energetic group of local historians who can look with confidence for municipal support. This happy combination promises a bright future to local historical and archaeological studies in Leicestershire. F. T. WAINWRIGHT.